The Short- and Long-Term Effects of Graduating During a Recession: Evidence from Finland

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Abstract

Extensive evidence in economics shows that facing a recession upon entry to the labor market can have sizable and persistent effects on the earnings and careers of labor market entrants. Long-term negative effects have been found among young and low-educated workers, but also among highly educated labor market participants such as university graduates. Theory and empirical evidence suggest that the negative effects arise because of a prolonged period of job search and fewer opportunities early on in the career for finding employment that fits the worker's skills, for example. Identifying those most susceptible to persistent effects and understanding the mechanisms and channels underlying them are important for improving the effectiveness of active labor market policies and other public policy instruments as well as the school-to-work transition.

This thesis studies the short-term and long-term effects of facing adverse economic conditions upon graduation on real annual earnings, unemployment and other labor market outcomes among Finnish university graduates who obtained a Master's degree between 1988 and 2004. The empirical strategy uses idiosyncratic variation in regional unemployment rates as a proxy for regional business cycle fluctuation, controlling for common national business cycle fluctuation and regional fixed effects. The thesis contributes to the existing literature in three ways. First, it provides the first evidence on the effects of graduating from university upon adverse economic conditions in Finland. The results with Finnish data are compared to other countries with different labor market institutions. Second, the time period investigated in this thesis (1988-2014) includes a period unlike any other studied in the existing literature: the exceptionally deep 1990s Finnish depression. Third, it contributes to the relatively scarce evidence on gender differences in the effects of graduating into a recession.

The data used in this thesis contain matched employer-employee panel data on the first ten post-graduation years of around 140,000 graduates. The results show that facing a six percentage points (roughly a standard deviation) above average regional unemployment rate in the region of residence in the year of graduation on average reduces annual earnings by 12.6% in the following year after graduation. Remarkably, this initial effect is only halved after 9–10 years. These effects on earnings are larger than what have generally been found in the literature and are similar to those reported with U.S. and Canadian data, for example. Furthermore, there is a persistently higher probability of being unemployed that lasts for roughly seven years. Smaller and more short-lived effects are found when only considering cohorts who graduated after the 1990s depression: the effects on earnings last only for the first five years and there are no effects on unemployment. These findings suggest that under more normal business cycle fluctuation, mechanisms other than unemployment are responsible for the earnings losses. Given the relatively high levels of wage rigidity in Finland, the existing literature suggests that the earnings losses can result from task downgrading and skill mismatch, for example. Finally, the results show that the effects on earnings are smaller for female graduates, perhaps reflecting gender differences in fields of study, employing sector and labor market attachment. Robustness checks indicate that the empirical results are not likely to be affected by selective timing or place of graduation.

Keywords
labor market, unemployment, business cycle fluctuation, higher education

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Tiivistelmä

Taloustieteessä on laajaa näyttöä siitä, että taantuman kokemisella työmarkkinoille tulon yhteydessä voi olla mittavia ja pitkäkestoisia vaikutuksia ansioihin ja työuraan. Pitkäaikaisia negatiivisia vaikutuksia on löydetty niin nuorien ja vähän koulutettujen työntekijöiden kuin korkeakoulutettujenkin keskuudessa. Taloustieteellisen teorian ja empiirisen näytön perusteella nämä haitalliset vaikutukset voivat johtua esimerkiksi pitkittyneestä työnhausta tai vähäisemmistä mahdollisuuksista saada osaamista vastaavaa työtä työuran alussa. On tärkeää tunnistaa pitkäaikaisille vaikutuksille kaikkein herkimmät henkilöt ja ymmärtää vaikutusten taustalla olevat mekanismit, jotta niin aktiivista työvoimapolitiikkaa ja muita julkisen vallan politiikkakeinoja kuin opinnoista työelämään siirtymistä voidaan parantaa.

Tässä tutkielmassa tarkastellaan taantuman aikana valmistumisen lyhyen ja pitkän aikavälin vaikutuksia vuosina 1988–2004 yliopiston maisterin tutkinnon hankkineiden suomalaisten vastavalmistuneiden reaalisiin vuosiansioihin, työttömyyteen ja muihin työmarkkinatulemiin. Empiirisenä strategiana on hyödyntää maakuntakohtaista suhdannevaihtelua, jota mitataan idiosynkraattisella maakuntakohtaisen työttömyysasteen vaihtelulla, jossa on kontrolloitu koko maan yhteinen suhdannevaihtelu ja maakuntakohtaiset kiinteät vaikutukset (engl. fixed effects). Tutkielma edistää tutkimuskysymykseen liittyvää kirjallisuutta kolmella tavalla. Se ensinnäkin tarjoaa ensimmäisenä tutkimusnäyttöä yliopistosta huonon taloustilanteen aikana valmistumisen vaikutuksista Suomessa. Suomen aineistolla saatuja tuloksia vertaillaan muihin maihin, joissa työmarkkinainstituutiot ovat erilaiset. Toiseksi, tutkielmassa tarkasteltava aikaväli (1988–2004) sisältää poikkeuksellisen syvän 1990-luvun laman, jonka kaltaista ajanjaksoa ei ole tutkittu aiemmassa kirjallisuudessa. Kolmanneksi, tutkielma tuo oman panoksensa suhteellisen niukkaan tutkimusnäyttöön, joka koskee taantuman aikana valmistumisen vaikutusten eroja sukupuolten välillä.

Tutkielmassa käytetään yhdistettyä työntekijä-työnantaja-paneeliaineistoa, jossa on tietoa noin 140 000 vastavalmistuneen valmistumisvuotta seuraavista ensimmäisestä kymmenestä vuodesta. Tulosten mukaan kuusi prosenttiyksikköä (noin yhden keskihajonnan) keskimääräistä korkeamman maakuntakohtaisen työttömyysasteen kohtaaminen valmistumisvuonna asuinmaakunnassa alentaa vuosiansioita keskimäärin noin 12,6% valmistumista seuraavana vuonna. Tämä varhainen vaikutus yllättäen puoliintuu vasta 9–10 vuoden jälkeen. Nämä vaikutukset ansioihin ovat suurempia kuin mitä kirjallisuudessa on yleensä löydetty ja ne ovat samankaltaisia esimerkiksi yhdysvaltalaisilla ja kanadalaisilla aineistoilla löydettyjen vaikutusten kanssa. Lisäksi tulokset osoittavat, että työttömänä olemisen todennäköisyys on korkeampi noin seitsemän vuoden ajan valmistumisen jälkeen. Kun tarkastellaan vain 1990-luvun laman jälkeen valmistuneita, vaikutukset ovat pienempiä ja lyhytaikaisempia: vaikutukset vuosiansioihin kestävät vain viisi vuotta eikä vaikutuksia työttömyyteen ole lainkaan. Näiden löydösten perusteella vaikuttaa siltä, että tavanomaisempien suhdannevaihteluiden oloissa muut mekanismit kuin työttömyys ovat vuosiansioita koskevien vaikutusten taustalla. Ottaen huomioon, että Suomessa palkat ovat suhteellisen jäykkiä, vaikutukset ansioihin voivat aiemman kirjallisuuden mukaan johtua esimerkiksi työtehtävien huonontumisesta tai työllistymisestä osaamista vastaamattomaan työhön. Lopuksi, tulosten mukaan vaikutukset vuosiansioihin ovat pienemmät vastavalmistuneiden naisten keskuudessa, mikä voi johtua koulutusaloihin, työllistävään sektoriin tai työmarkkinakiinnittymiseen (engl. labor market attachment) liittyvistä eroista. Robustisuustarkasteluiden perusteella strateginen valmistumisen viivästyttäminen tai asuinpaikan strateginen valitseminen valmistumisen yhteydessä eivät näytä vaikuttavan empiirisiin tuloksiin.

Avainsanat
työmarkkinat, työttömyys, suhdannevaihtelut, korkeakoulutus

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Chapter 1

Introduction

Each year approximately 12 000 – 15 000 students graduate with a higher university degree and approximately 20 000 – 25 000 students graduate with a polytechnic degree in Finland.[1] For some of these graduates, the prevailing economic conditions upon graduation offer more opportunities for finding a good job and pursuing a career than for others. Graduates about to enter the labor market during recessions can be especially concerned. How and for how long will recessions affect them? Do those graduating during recessions fare worse in the labor market than those graduating just before or after recessions? If so, in what ways do these disparities arise? These are some of the questions I try to answer in this thesis.

Previous research in economics has highlighted the importance of economic conditions at the time of labor market entry for subsequent labor market outcomes. According to several studies, cohorts entering the labor market in times of adverse economic conditions can have, on average, lower wage levels and annual earnings, lower-level initial jobs and a higher probability of being unemployed even many years after entry compared to cohorts entering in better economic conditions. This suggests that even temporary differences in economic conditions can result in arguably unfair long-run disparities between "lucky" and "unlucky" cohorts. These persistent negative effects have been found, for example, among young workers, blue-collar workers, and workers with at most a high-school degree.[2]

However, in this thesis I focus on the effects of initial economic conditions on university graduates entering the labor market. By now, there is extensive empirical evidence showing that adverse labor market conditions at the time of graduation can have long-lasting effects on labor market outcomes. For example, Oreopoulos et al. (2012) use

[1] Source: Statistics Finland. A higher university degree is equivalent to a Master's Degree. A degree from a polytechnic school (also called a university of applied sciences, or in Finnish *ammattikorkeakoulu*) is comparable to a lower (bachelor's) university degree.

[2] See, for example, Gardecki and Neumark (1998), Ellwood (1982), Neumark (2002), Burgess et al. (2003), Raaum and Røed (2006), Umkehrer (2015), and Liu et al. (2014).

Canadian administrative data and find that students graduating during recessions on average suffer earnings losses that persist for as much as ten years.[3] Previous research has also found considerable heterogeneity in susceptibility to the effects of adverse initial labor market conditions between students with different college majors, for example (see Altonji et al., 2016). Understanding the magnitude and persistence of these effects, recognizing the mechanisms behind them, and identifying those most at risk of long-term adverse effects are essential for the design of effective policy interventions and school-to-work and employment programs.

In this thesis, I study the short- and long-term effects on labor market outcomes of graduating from university under adverse economic conditions in Finland. My sample consists of individuals who obtained a Master's degree between 1988 and 2004 and were aged 22–35 in the year of graduation. Given the recent challenging economic conditions in Finland, this research topic has gained some coverage in the Finnish media.[4] To my knowledge, this thesis provides the first attempt at studying these issues using Finnish data. I use matched employer-employee panel data provided by Statistics Finland which includes information on for example the employment status, earnings, and educational attainment of university graduates. Using Finnish data is interesting for at least two reasons. First, the data include substantial business cycle fluctuations, especially the unusually deep depression Finland experienced in the beginning of the 1990s. Studying this time period is a relevant addition to the existing literature. It also includes the economic boom following the 1990s depression and the boom prior to the the current period of sluggish economic growth which started in 2008. Second, the labor markets in Finland (and in the Nordic countries in general) differ from the more flexible labor markets in the United States, for example. It is therefore interesting to compare the magnitude and persistence of the effects of adverse initial labor market conditions in different institutional environments.

I find that facing adverse economic conditions upon graduation from university can cause sizable and persistent negative effects on labor market outcomes. For example, an average graduate who faces a six percentage points (corresponding roughly to one standard deviation) higher regional unemployment rate in the year of graduation has roughly 12.6% lower real annual earnings in the year following the year of graduation. For the whole sample, this initial effect is halved only after 9–10 years. When only the cohorts who graduated after the deep 1990s depression are considered, the effects on earnings are smaller in magnitude. The effects are also less persistent, being limited to the first 5–6 years following graduation. The effects on earnings I find for all cohorts are in keeping with previous evidence from the United States, Canada and Belgium. In

[3]Studies using data U.S. data include, for example, Kahn (2010), Altonji et al. (2016), and Oyer (2006). Studies with European data include, for example, Brunner and Kuhn (2014) (Austria), Cockx and Ghirelli (2016) (Belgium), and Liu et al. (2016) (Norway). See Section 2.2.

[4]For example, a recent article in *Helsingin Sanomat* (see Puttonen 2016, in Finnish), one of the largest subscription newspapers in Finland, addresses the concerns of students about to graduate in the current economic situation.

comparison, the smaller effects found for cohorts who did not face the 1990s depression, and thus generally faced more normal business cycle fluctuation, are more in keeping with evidence from e.g. Norway, a rather similar country in terms of labor market features.

For the whole sample, I also find persistent negative effects on unemployment which last roughly for the first seven years after graduation. This results is consistent with evidence from Norway, but contrasts with evidence from previous studies that use Canadian and U.S. data that find little to no effects on employment. These disparities could reflect institutional differences with respect to wage rigidity and unemployment insurance system, for example. However, the effects I find are driven entirely by cohorts who faced the 1990s depression, suggesting that the major mechanisms behind the negative effects on earnings in more stable economic conditions are not related to unemployment, but instead lie elsewhere. Finally, I contribute to the relatively scarce evidence on the gender differences in the effects of facing adverse initial economic conditions. I find that the effects on earnings are larger and more persistent for male graduates than for female graduates, perhaps reflecting differences in fields of study, employing industries, and labor market attachment, for example. All my empirical results are insensitive to e.g. various alternative variable definitions and model specifications. They are also unlikely to be affected by selective timing and place of graduation.

The rest of this thesis is organized as follows. In Chapter 2, I present previous empirical evidence on the effects of adverse economic conditions at labor market entry on labor market outcomes. I also present some theoretical models which help explain why these effects arise. In Chapter 3, I describe the data set, sample construction and the main variables used in the analyses. After that I briefly discuss some relevant features of the institutional environment and time period of the study in Chapter 4. In Chapter 5, I describe the empirical strategy and discuss its advantages and potential problems. In Chapter 6, I present the main empirical results and relate them to the existing literature. I also perform various heterogeneity analyses as well as a range of sensitivity and robustness checks. Finally, Chapter 7 concludes and suggests topics for further research.

Chapter 2

Literature Review

This chapter presents an overview of the literature on how adverse economic conditions at labor market entry affect labor market outcomes. Section 2.1 reviews the theoretical literature, emphasizing two classes of models that explain why a brief recession at labor market entry could produce persistent negative effects: labor market search models with search frictions that increase with tenure length and age, and career progression models with on-the-job human capital accumulation. I abstain from providing a formal treatment of these theoretical models in order to keep the discussion brief. Section 2.2 reviews the empirical literature and compares results across different countries and institutional environments, and between university graduates and other types of labor market entrants.

2.1 Theory

Job Shopping

An extensive literature in economics looks at the effects of entering the labor market under adverse economic conditions. In the short run, the negative effects of entering the labor market in a recession or depression are evident: there are fewer jobs available, these jobs may be less attractive and may not match the job-seeker's skills well etc. Thus those entering the labor market in a bad economy may initially face a higher probability of being unemployed or underemployed. They may also be more likely to experience job mismatch, i.e. being forced to choose a job or career path they would not have picked had they entered the labor market at a better time.

However, it may not be as clear why adverse initial economic conditions would also affect long-run labor market outcomes. If labor market entrants are able to move easily to the jobs they would have chosen had they entered in better economic conditions, and

there are diminishing marginal returns to work experience, their disadvantage might prove to be small because they could quickly catch up with their luckier counterparts (Kahn, 2010, p. 304). This view is consistent with the evidence that "job shopping" is an important component of the early labor market experience.[5] As the theoretical labor market search models of e.g. Jovanovic (1979) and Johnson (1978) describe, job shopping can be explained if jobs are assumed to be so-called "experience goods".[6] This means that job-seekers cannot observe their ability and the quality of a potential labor market match perfectly *ex ante* but the accuracy improves over time by trying out different jobs. Because young workers might lack accurate information on their abilities at the outset of their careers, early jobs could prove to be bad matches but match quality improves upon subsequent employments, which results in increasing wages. These models thus predict that early-career job shopping is efficient and beneficial for wage growth, and that it shields unlucky cohorts entering the labor market during a recession against persistent losses.

Some studies find that job shopping indeed benefits early-career wage growth, but the overall evidence is mixed.[7] For example, Topel and Ward (1992) use U.S. longitudinal employer-employee data to study the post-entry labor market experiences of roughly 10,000 young men. They find that, on average, around two thirds of the total number of job changes take place in the first ten years of the career, and about a third of the rapid wage increase during the same time period is attributable to job shopping. Bartel and Borjas (1981) also find evidence that labor turnover positively affects wage growth with U.S. data from the National Longitudinal Survey of Young and Mature Men for the period 1967–1973, although the effect is smaller in magnitude. Gardecki and Neumark (1998), on the other hand, use U.S. data from the National Longitudinal Survey of Youth for the period 1979–1992 and find that a more stable early labor market experience does not improve labor market experiences (including wage levels), suggesting that initial periods of "churning" in the labor market do not have negative effects.

Search Theory

As I will discuss in Section 2.2, empirical evidence shows that the negative effects of poor initial labor market conditions actually seem to persist. A number of explanations

[5] Johnson (1978, p. 261) defines job shopping as "the period of experimentation with jobs and accompanying high rates of mobility, which typically occurs at the beginning of the working life".

[6] Roughly speaking, labor market search models study agents who maximize their expected lifetime utility. Agents can exert different intensities of effort in job search and this incurs different amounts of search costs. For excellent treatments on the use of search theory in labor economics, see e.g. Pissarides (2000) and Rogerson et al. (2005).

[7] Based on a cross-country comparison study, Ryan (2001, pp. 56–60) suggests that there may be a positive relationship between labor turnover and wage growth. However, finding a causal effect is challenging because of e.g. potential sample selection: those moving between jobs may be positively or negatively selected based on unobservable characteristics (see also Neumark, 2002, pp. 466–469).

for this have been proposed in the theoretical literature. First, search theory implies that job-seekers search longer for good matches because recessions worsen the job offer distribution by decreasing the number and quality of open vacancies. There is also evidence suggesting that search costs increase with age and tenure length.[8] Thus some workers entering the labor market during recessions and starting off in low-grade jobs (that they would not have chosen at better times) might stop their job search if it takes too long and the benefits of staying in the current job outweigh the expected benefits of continuing the search. In this case adverse economic conditions at labor market entry may incur permanent losses if the low-grade initial jobs do not offer similar possibilities for career progression.

Following the reasoning above, Oreopoulos et al. (2008, pp. 6–16) present a labor market search model with endogenous search intensity. In their model, workers are either of low-skill (i.e. low productivity) or high-skill (high productivity) type, and there are high and low productivity firms. High productivity firms always pay higher wages than low productivity firms (for either type of worker), and it is assumed that high productivity firms pay higher wages for high-skill workers, wages are deterministic, and that the wage level in both firm types is an increasing concave function of job tenure length.[9] If a worker gets a job in a high productivity firm, she no longer has incentives to search for a job, whereas workers employed in low productivity firms may search for a job in a high productivity firm. Search costs are assumed to be increasing and convex in search intensity and increasing in age. A short-term adverse labor market shock is defined as a temporary decrease in the hiring rate of high productivity firms.

Several insights arise from the model. As the tenure length of a low productivity firm employee increases, the benefits from job search decline and the optimal level of search intensity decreases. Thus the convergence of wage levels between those entering during a recession and those entering during normal times is slower the longer workers stay in low productivity firms. Also, the number of low productivity firm employees who give up job search completely increases over time. This means that there is a growing number of workers who suffer permanent losses because of adverse initial economic conditions. The model also predicts that recessions give rise to heterogenous effects. On the one hand, the catch-up in wage levels is quicker for high-skill workers because they search more intensively and thus are more likely to move to a high productivity firm. This is because they value job search more as they are better paid in high productivity firms. On the other hand, low-skill employees in low productivity firms give up job search

[8] Search costs can refer to literal monetary costs but also e.g. to the disutility of decreased leisure time due to time spent on job search. For example, Bloemen (2005) finds using Dutch household panel survey data on employed and unemployed men that the cost of search decreases with age until the age of 29 after which the costs of search increase. These observations could reflect that job search becomes less attractive as labor market participants settle down because of starting a family or buying a house, for example.

[9] The last assumption implies that the wage level is higher the longer the worker has been employed in the firm, but the magnitude of the wage increase for each additional year declines with tenure length. This allows catch-up to occur: the wage level of a new employee in either firm type approaches the wage level of more experienced workers within the same firm over time.

earlier than high-skill workers and therefore are more likely to suffer permanent losses. Thus the model predicts that, on average, high-skill workers bounce back faster and suffer less from negative shocks upon labor market entry.

Disparities in Human Capital Accumulation

Another explanation to the persistent effects of adverse labor market entry conditions is disparities in on-the-job human capital accumulation between cohorts. To explain the mechanism behind this, Gibbons and Waldman (2006) develop a career-progression model incorporating the possibility of accumulating task-specific human capital.[10] As mentioned earlier, job-seekers entering in recessions face a slacker labor market and thus have a higher probability of being unemployed, underemployed and experiencing skill mismatch in employment. In addition, the supply of high-wage jobs and jobs with good possibilities for career progression is likely to be scarce during recessions (see e.g. McLaughlin and Bils, 2001). In recessions job-seekers are thus more likely to begin their career in lower-grade tasks and jobs and are therefore likely to have less opportunities to accumulate industry-, job- or task-specific human capital than those entering at better times. This implies that the starting wages of these cohorts are likely to be lower. When the workers eventually move to more attractive tasks (within the same or another firm), some of the previously accumulated human capital cannot be utilized. As luckier cohorts are more likely to start off in better tasks and accumulate more relevant human capital, recessions cause wage and earnings differentials between workers, conditional on work experience.

It is also possible that when workers who enter the labor market during recessions start off in lower-grade tasks, they face worse promotion paths than workers who enter in better times and start off in better tasks, leading to persistent wage differentials. Yet another possibility, that is especially relevant to university graduates, is that recessions may force unlucky cohorts to involuntarily accept jobs and tasks ill-suited to them and to invest in wrong types of industry-, job- or task-specific human capital because of a reduced supply of jobs that match well with their skills (e.g. their field of study) (Kahn, 2010, pp. 304–305).[11] Part of these investments are wasted when some of these workers eventually find a better employment match.

Regardless of the relevant mechanism, the model described above predicts that the early-career human capital disparities caused by business cycle fluctuations can produce cohort effects (see e.g. Baker et al. 1994, and Beaudry and DiNardo 1991): the wage levels of workers whose starting wages are lower due to entering the labor market

[10] Task-specific human capital refers to the knowledge and skills a worker gains on the job that are tied to the tasks she performs. Task-specific human capital is thus not fully applicable when performing other tasks within the same firm or industry. See, for example, Gibbons and Waldman (2004).

[11] For example, Liu et al. (2016) find with Norwegian data on college graduates that skill mismatch has a countercyclical pattern. See also Section 2.2.

during recessions continue to be lower even several years later. In other words, worse opportunities for on-the-job human capital accumulation during recessions are causing the persistent long-run effects. According to the model, the persistent negative effects of recessions should exist not just between industries and firms, but also within the same job and occupation (because task-specific human capital is included). In the context of the model, heterogeneity in the persistence of the effects can stem, for example, from the fact that industries differ with respect to the cyclicality of their labor demand.[12]

In summary, the two theoretical models presented in this section emphasize several channels through which recessions at labor market entry produce persistent negative effects: occupational and/or task down-grading, reduced opportunities to accumulate relevant kinds of human capital, and increasing costs of job search. In addition, mobility between firms and moving to better tasks (perhaps within the same firm) are seen as the primary ways of catch-up for unlucky labor market entrants.

2.2 Empirical Studies

In this section, I review the empirical evidence on the magnitude and persistence of the negative effects of adverse labor market entry conditions from studies mainly using North American and European data. The regression model used in the literature for estimation usually takes the following "generic" form:

$$y_{icrt} = \alpha + \sum_{e=1}^{T} \beta_e \, UR_{cr0} \;+\; \phi_t \;+\; \theta_r \;+\; \gamma_e \;+\; \chi_c \;+\; u_{icrt},$$

where α is the constant term, u_{icrt} is the error term, and $\phi_t, \theta_r, \gamma_e$, and χ_c denote fixed effects with respect to year, region (state, county, province etc.) of residence/college in the year of graduation, potential work experience (i.e. years since graduation), and graduation cohort, respectively.[13] The coefficient of interest, β_e, measures the effect of a percentage point increase in the regional unemployment rate in the year of graduation (UR_{cr0}) on the labor market outcome variable (y_{icrt}) in year t of individual i who belongs to graduation cohort c and for whom the region of residence/college in the year of graduation is r. The effect is allowed to variate by years since graduation ($e = 1, ..., T$) in order to assess the persistence of the effect. Taking out the year effects and regional fixed effects should produce plausibly exogenous variation in the regional unemployment rate (which is used as a proxy for overall regional economic conditions)

[12] For example, college graduates majoring in education, health, or other majors where public sector is the primary employer, can be more shielded from business cycle fluctuations (Liu et al., 2016).

[13] This "generic" regression equation is given mainly to make the understanding of this section easier. There is of course some variation across studies in the exact model specification in terms of including additional control variables, using variable trends or fixed effects etc. One notable difference is that some studies work with individual-level observations (e.g. Kahn 2010, Cockx and Ghirelli 2016) while others work with grouped data with groups defined by graduation cohort and region of residence/college in the year of graduation (e.g. Oreopoulos et al. 2012, Liu et al. 2016).

at labor market entry. I provide a more thorough discussion of the empirical strategy used in the literature, and in this thesis in particular, in Chapter 5.

The most relevant studies presented in this section are summarized in Table 1, with studies using North American and European data collected in Panels A and B, respectively. Each row summarizes the context of the study, main outcome variables, main unemployment variables used as regressors, and selected estimates for some of the outcome variables (with standard errors in parentheses). Finally, I also briefly discuss the relationship between labor market institutions and the persistence of the effects and review the relevant mechanisms behind the effects.

North American Evidence

Oreopoulos et al. (2012) use Canadian administrative university-employer-employee data on male college graduates who graduated between 1976 and 1995 (see the first row of Table 1, Panel A). They find that a rise in the regional (here, provincial) unemployment rate by five percentage points (indicating a typical recession in their sample) upon graduation lowers the annual earnings of an average graduate initially by roughly 9% one year after graduation. The effect halves within five years and becomes statistically insignificant after ten years. This implies a 5% decrease in cumulative earnings during the first ten years. Remarkably, when they control for the subsequent history of unemployment rates, they find that these effects are mainly due to the very first unemployment rate graduates face at labor market entry.[14]

Altonji et al. (2016) use a large pooled U.S. data set drawn from the Current Population Survey and from a combination of several other surveys to study the first 13 years of the careers of college graduates who graduated between 1974 and 2011 (see the second row of Table 1, Panel A).[15] Exploiting annual unemployment rate variation in the nine U.S. Census divisions, they find similar results to Oreopoulos et al. (2012): for the average graduate, a typical recession (where unemployment rate rises by four percentage points) reduces annual earnings by 10% in the first year after graduation. The effect halves within three years and disappears within the first 7–10 years. This implies an average *per-year* loss in earnings of 1.6% during the first ten years after graduation. The earnings loss is driven by a combination of reductions in wage rates and working

[14]Controlling for subsequent unemployment rates is sensible because the regional economic conditions graduates face upon graduation tend to be correlated with the conditions they face in later years. Thus estimates of the effects of unemployment rates at labor market entry *without* controls for subsequent unemployment rates capture both the effects of the initial unemployment rate *and* the effects of a regular subsequent unemployment rate history (Oreopoulos et al., 2012, p. 7, footnote 11). See also Section 6.4.

[15]The complete list of data sets used in Altonji et al. (2016, pp. S366–S367): the National Longitudinal Survey of Youth (NLSY79 & NLSY97), the National Survey of College Graduates (NSCG93 & NSCG03), the Baccalaureate and Beyond (B&B93 & B&B08), the National Longitudinal Study (NLS72), the Survey of Income and Program Participation (SIPP) 1984–2008 panels, and the American Community Survey (ACS, years 2009–2012).

Table 1: Summary of the Main Empirical Studies

Panel A: North American Studies

Study	Context	Main Outcome(s)	Unemployment Variable(s)	Main Estimates (Incl. Std. Errors)
Oreopoulos et al. (2012)	Canada, matched employer-employee data. Male college graduates, cohorts 1976–1995.	Log real annual earnings	Provincial (& national) UR	Earnings (provincial UR): –0.0183 (0.0020) [0–1 years after graduation] –0.0089 (0.0016) [4–5] –0.0042 (0.0017) [9–10]
Altonji et al. (2016)	United States, pooled data from several survey data sources. College graduates, cohorts 1974–2011.	Log real annual earnings, probability of being employed/ working full-time, log real wage rate	Census Division UR	Earnings: –0.1182 (0.0239) [1 year after graduation] –0.0402 (0.0141) [3] Full-time employment: –0.0663 (0.0119) [1] –0.0274 (0.0071) [3] Wages: –0.0425 (0.0197) [1] –0.0146 (0.0111) [3]
Kahn (2010)	United States, survey data from the NLSY79. White-male college graduates, cohorts 1979–1989.	Log real hourly wage rate, annual weeks worked	State-level & national UR	Wages (national UR): –0.059 (0.020) [1 year after graduation] –0.050 (0.014) [5] –0.038 (0.010) [10] –0.026 (0.012) [15]

Table 1: Summary of the Main Empirical Studies (*Continued*)

Panel B: European Studies

Study	Context	Main Outcome(s)	Unemployment Variable(s)	Main Estimates (Incl. Std. Errors)
Liu et al. (2016)	Norway, college graduates, cohorts 1988–2003	Log real annual earnings, probability of unemployment	County-level UR	Earnings: –0.061 (0.008) [1–2 years after graduation] –0.025 (0.007) [3–4] –0.008 (0.007) [5–6] Unemployment: 0.014 (0.002) [1–2] 0.005 (0.002) [3–4] 0.005 (0.002) [5–6]
Cockx and Ghirelli (2016)	Belgium, Flanders region, matched survey and administrative data. Low-educated (high school or less, cohorts 1994–2001) and high-educated (beyond high school, cohorts 1997–2004) young men.	Log real annual earnings, log average real hourly wage, log annual hours worked, overall (salaried/self-employed) employment	Provincial & aggregate UR (for Flanders)	Earnings, high-educ. (provincial UR): –0.058 (0.019) [1 year after graduation] –0.032 (0.013) [5] –0.043 (0.014) [10] Employment, high-educ. (provincial UR): –0.015 (0.006) [1] –0.011 (0.004) [3] –0.004 (0.002) [5]

Notes: The main estimates measure changes in the outcome variable due to a percentage point change in the relevant unemployment rate in the year of graduation. The only exception is Altonji et al. (2016), where unemployment rate is assumed to increase by 4 percentage points (a large recession).

Exact sources for the estimates: Oreopoulos et al. (2012): Table 1, Panel A, column (3); Altonji et al. (2016): Table 3, Panel A, columns (1), (3) & (4); Kahn (2010): Table 4, Panel B, column (1) ; Liu et al. (2016): Table 2, columns (1) & (4); Cockx and Ghirelli (2016): Table C.1, columns (3) & (6). See Section 2.2 for more discussion.

hours. Interestingly, the impacts on the probability of being employed are modest, but the probability of being employed full-time decreases. In addition, there seems to be a small persistent decrease in occupational quality.

Kahn (2010) uses a smaller U.S. dataset from the National Longitudinal Survey of Youth (see the third row of Table 1, Panel A). She only studies white males who graduated from college in 1979–1989 and uses variation in both state-level and national unemployment rates. Consistently with the aforementioned studies, she also finds negative wage effects: for example, a percentage point increase in national unemployment rate decreases the average wage by 6–7% one year after graduation, but the loss is still 2.5% even after 15 years. These wage effects are notably larger and more persistent than those reported by Altonji et al. (2016) and Oreopoulos et al. (2012). However, as Altonji et al. (2016) and Oreopoulos et al. (2012) note, this can be due to the small sample size limiting statistical power (only 513 individuals in a panel of 11 graduation cohorts), the time period studied (which includes the severe U.S. recession of the early 1980s), and the focus on only white males, a group generally with strong labor market attachment. Similarly to Altonji et al. (2016), Kahn (2010) finds that labor supply is mostly unaffected. Consistently with the theoretical predictions of Section 2.1, she also finds that the national unemployment rate is slightly positively correlated with tenure length and slightly negatively correlated with occupational attainment.

Studies using North American data find considerable heterogeneity in the effects of recessions on labor market outcomes. Oreopoulos et al. (2012) divide graduates into more and less advantaged groups by estimating predicted earnings using information on the college attended and chosen field of study. The effects vary substantially; for example, the most disadvantaged graduates suffer roughly an 8% loss in cumulative earnings during the first 10 years when graduating into a recession – twice the loss of the median graduate and over four times larger than the loss of top graduates. On the other hand, Altonji et al. (2016) distinguish between the experiences of different graduates by calculating the earnings premia of different college majors. In their study, graduates in majors with earnings premia that are one standard deviation *above* the mean experience roughly *half* the earnings loss of graduates in majors with mean earnings premia. Furthermore, graduates in majors with earnings premia that are one standard deviation *below* the mean face roughly *50% larger* earnings losses. Graduates in high-paying majors thus increase their earnings advantage over other majors during recessions. These heterogeneities are driven by differences in effects on employment, full-time employment, wage rates, and occupational attainment.[16]

In sum, the North American evidence indicates that graduating into a recession has sizable and persistent negative effects. The negative effects on earnings arise mainly

[16]Naturally, these results partly reflect endogenous selection by e.g. ability into different college majors and industries. However, given the magnitude of these heterogeneities, selection is unlikely to fully account for the differences.

through occupational downgrading and reductions in wage levels, full-time employment and working hours. Furthermore, any negative effects on the probability of being employed mostly appear to be small and short-lived. However, it is questionable how well these results carry over to other countries with different labor market institutions. For example, in countries with high trade union density and a centralized wage-setting system wages may be considerably more rigid downwards, and thus we could expect recessions to affect more through a higher probability of being unemployed than through wage reductions. Thus it is important to study how labor market institutions are related to the results. I provide an overlook of the Finnish labor market system later in Section 4.2.

Influence of Labor Market Institutions

Some studies directly compare the negative effects of recessions at labor market entry between countries with different labor market institutions. In general, these studies suggest that a more rigid labor market tends to be associated with more persistent effects. However, it should be emphasized that, due to the endogeneity of labor market institutions, this evidence alone cannot be used to form a causal relationship. For example, Kawaguchi and Murao (2014) perform a cross-country study with panel data from 20 OECD countries for the period 1960–2010. They form a composite index of labor market rigidity using variables measuring the degree of employment protection legislature, labor union coverage rate and the generosity of the unemployment insurance system, and find that in countries with more rigid labor market institutions, there is a stronger positive relationship between unemployment rate at ages 15–24 and unemployment rates at ages 25–29 and 30–34.

Genda et al. (2010) compare the effects of recessions at labor market entry on more-educated (college degree or higher) and less-educated males (high-school degree at most) in the U.S. and Japan using comparable labor force survey data from both countries. They find that a recession at entry has persistent negative effects on the probability of being employed (driven by a reduction in full-time employment) for less-educated Japanese men, while in the U.S. there are only small and temporary effects.[17] They attribute this difference to the stark institutional differences between the two countries. In Japan, a school-based hiring system of regular, full-time workers and strict dismissal regulations protecting regular workers make it more favorable for potential employers to prefer graduating high school seniors over older graduates in hiring.[18] These features also create a clear distinction between regular workers and part-time provisional workers.

[17] For more-educated workers, the authors find that the negative effects are somewhat larger in Japan as well, but these are not due to a decline in full-time employment.

[18] The hiring system requires schools to act as mediators between graduating high-school seniors and potential employers. If employers would want to hire workers with similar educational attainment who have already graduated earlier, they would have to screen the applicants themselves. For college-educated workers, no such hiring system exists. (Genda et al., 2010, Section II)

In addition, they favor long-term employment over frequent job changes, making it harder for the less-educated to find a full-time job if they are unable to find one initially, thus potentially causing longer job search and unemployment spells. In comparison, the U.S. labor market is far more flexible: there are no clear differences between full-time stable employment and part-time employment, and dismissals are rarely costly. The U.S. labor market thus allows early-career job shopping more easily.

European Evidence & State Dependence Literature

European labor markets are generally seen as more rigid than those in North America (see e.g. Nickell, 1997). This difference suggests that in Europe the effects of recessions could be more persistent and that unemployment should be a more relevant adjustment channel. Some studies using European data partly support these hypotheses. For example, Raaum and Røed (2006) use Norwegian data to estimate the causal effect of early local labor market conditions on adult unemployment and non-employment. They find that, in comparison to cohorts facing an economic boom, cohorts who experience a recession at ages 16–19 may face an unemployment rate that is persistently 1–2 percentage points higher during their prime working years (ages 25–36). Given the generally low rates of unemployment in Norway, this difference is economically significant.

Closely related to this thesis, Liu et al. (2016) study the effects of graduating into a recession with Norwegian register data on cohorts of college graduates who graduated between 1988 and 2003 (see the first row of Table 1, Panel B). Given the broad similarities between the Norwegian and Finnish labor market institutions (see Section 4.2), these results give a useful point of comparison for my findings with Finnish data. The authors find that a percentage point increase in the regional unemployment rate leads to an initial drop in earnings of 6.1 % during the first two years after graduation, but the effect vanishes already after five years. The effect on earnings thus seems to be less persistent than in studies with North American data.

Contrary to the North American evidence, the effect on unemployment is more salient: a percentage point increase in the regional unemployment rate increases the probability of being unemployed by 1.4 percentage points in the first two years after graduation. The effect gradually dissipates but remains significant at the 5% level 5–6 years after graduation (and still remains significant at the 10% level even after 7–8 years). As the authors discuss, this finding suggests that graduates initially favor voluntary unemployment over accepting worse-paying jobs because of the extensive social assistance and unemployment protection programs available.[19] The results are driven largely by graduates in majors where private sector is the main employer. In comparison, graduates in majors related to education and health care, for which public sector is the primary

[19] Unfortunately, Liu et al. (2016) cannot further distinguish between the different channels (working hours, full-time employment etc.) through which the effects of recessions arise.

employer and labor demand is likely to be more inelastic, face similar initial effects upon graduation but the effects die out after two years.

Brunner and Kuhn (2014) use Austrian individual-level administrative data on young men (aged 16–21) working in the private sector who first entered the labor market between 1978 and 2000. Contrary to the previously mentioned studies, the authors focus on low-to-medium skilled workers and study both white- and blue-collar workers. For the whole sample, they find that a percentage point increase in the initial state unemployment rate after labor market entry is associated with a slight 0.9 % decrease in the real daily wage. Remarkably, this effect persists even after 20 years and the loss appears to slightly increase over time. These results amount to a life-time wage loss of roughly 1–1.6 %.[20] The persistence of the negative effects is consistent with the rigidity of the Austrian labor market that has a relatively high level of unemployment protection and a centralized wage bargaining system. Moreover, for white-collar workers the effects disappear within 5–10 years, whereas the effects are much more persistent for blue-collar workers.

Cockx and Ghirelli (2016) use survey data matched with administrative data on Flanders, the largest region in Belgium (see the second row of Table 1, Panel B). They compare effects of a recession upon labor market entry between low-educated (high school or less) and high-educated (beyond high school) young men (aged 18–24). For the high-educated, they find very persistent effects on annual earnings. Earnings fall by 5.8% due to a percentage point increase in the provincial unemployment rate one year after graduation. Since in their data a typical recession indicates a 1.4 percentage points increase in the unemployment rate, the authors note that this initial effect (–8.1%) is similar in magnitude to the effect found by Oreopoulos et al. (2012) for Canada. Remarkably, the effect on earnings persists and is statistically significant for the first ten years after graduation, with the negative effect of a typical recession indicating a loss of roughly between 4.2% and 5.6% each year. For the low-educated, the effects on earnings are smaller but similarly persistent. For the high-educated, Cockx and Ghirelli (2016) find that the negative effects on earnings arise through reductions in hourly wages rather than reductions in hours worked (or employment). For the low-educated, the hours worked instead adjust downwards but wages are largely unaffected.

Cockx and Ghirelli (2016) suggest that their results are explained by institutional details of the rigid Belgian labor market. For the low-educated, who are primarily in blue-collar jobs, the high minimum wage is likely to be binding and generous unemployment insurance and short-time compensation systems reduce labor market mobility, causing the persistent negative effects to arise through a prolonged higher risk of unemployment. The high-educated, who are primarily in white-collar jobs, are instead shielded against

[20]The authors note that their estimates are actually likely to be biased downwards because they find some evidence of positive sample selection during recessions (i.e. more able individuals are more likely to find jobs during recessions).

unemployment by a strong employment protection legislature, causing the adjustment to arise through wage reductions instead.

Somewhat contrasting the previously mentioned studies, Umkehrer (2015) documents large and permanent negative effects of recessions upon labor market entry among medium-skilled youth who graduated from the German dual apprenticeship system in 1992–2009. The author finds that these effects arise through reduced real wage levels; the effects on labor market supply (days of employment and unemployment) are modest at most. The heterogeneity of the effects among more and less advantaged groups of graduates is also notable: the effect on real annual earnings for the less advantaged can be twice as large as for the more advantaged. However, the effects on earnings seem to be permanent even for the more advantaged.

Considering the evidence showing that the persistent negative effects of recessions may arise through their effects on employment, an important related branch of research for this thesis is also the literature on the "scarring effects" or "state dependence" of unemployment. According to this literature, even short unemployment spells experienced early on in the labor market can have negative long-run effects on labor market outcomes. These effects can arise because of a persistent higher probability for being unemployed and underemployed, slower accumulation or even depreciation of human capital (e.g. Pissarides, 1992), psychological distress and discouragement caused by early unemployment, and negative signals conveyed by unemployment spells to potential employers (referred to in economics as "negative duration dependence").[21] For example, Mroz and Savage (2006), Burgess et al. (2003), Nordström Skans (2011), and Ellwood (1982) document persistent – but not necessarily permanent – negative effects of early unemployment experiences on earnings and employment. There are also some studies pertinent to this literature that use Finnish data and especially look at the effects of the deep depression Finland went through in the beginning of the 1990s (see Section 4.4). For instance, Hämäläinen (2003) studies the labor market outcomes of individuals who graduated or finished compulsory education in 1988 for the period 1989–1998. He finds that, on average, an unemployment spell experienced two years earlier increases the current probability of being unemployed by as much as 20 percentage points. Interestingly, for university students the effect is much more modest (only 3–4 percentage points), suggesting that they can shake off past unemployment experiences even in unfavorable economic conditions.

[21] For recent empirical evidence on negative duration dependence from the U.S., see e.g. Kroft et al. (2013) who perform an audit study where fictitious CVs with varying information on employment status and length of current unemployment spell are sent to real employers.

Mechanisms

Finally, empirical evidence supports the theoretical literature on the relevant mechanisms through which the negative persistent effects of recessions at labor market entry arise. First, the quality of the first job placement seems to play an important part. For example, Oreopoulos et al. (2012) conclude that roughly 40–50 % of the negative persistent effects are attributable to the fall in firm size of and the average wage paid by the first employer. Furthermore, they show that the recovery from early labor market setbacks appear to happen in two ways. On the one hand, graduates entering the labor market during recessions increase their mobility between firms and industries. Consistently with their search theoretic model presented in Section 2.1 (see Oreopoulos et al., 2008), this channel is especially important for the most advantaged graduates who benefit the most from active job search. However, the most disadvantaged graduates seem to be stuck in lower-grade jobs and thus may suffer permanent losses during recessions.

Umkehrer (2015, pp. 21–24) also shows that the quality of first employment plays a major part in explaining the negative effects of recessions in his sample of medium-skilled German youth. On average, unlucky cohorts are initially employed in less stable jobs, in smaller firms and firms with a lower median wage paid to employees. Somewhat contrasting the results obtained for more-educated workers by Oreopoulos et al. (2012), Umkehrer (2015) finds that subsequent job mobility and spatial mobility do not allow unlucky cohorts to fully recover from the adverse economic conditions they faced upon labor market entry.

As discussed in Section 2.1, another possible mechanism suggested by theory is that workers entering the labor market during recessions face worse promotion paths than those entering during booms. There is some empirical evidence in favor of this hypothesis. For example, Kwon et al. (2010) study cohort effects in promotion patterns among private sector white-collar workers using Swedish matched employer-employee data for the period 1970–1990. Remarkably, they find that the promotion paths are pro-cyclical, even after controlling for educational attainment (as a proxy for productivity) and initial job placement (occupation and rank).[22] This implies that the promotion patterns of workers who start off in same-level jobs may differ by the timing of labor market entry. Since type of initial job placement is controlled for, the authors note that this phenomenon cannot be fully explained by the model of on-the-job human capital accumulation of Gibbons and Waldman (2006) presented in Section 2.1. Furthermore, the authors' findings of differences in promotion paths can partly explain the cohort effects in wages they observe.

[22]Kwon et al. (2010, Section VI) complement these findings with evidence from a case study of workers of a single occupation (health insurance claim processor) in a single U.S. insurance firm. These personnel data contain comparable performance measures created and used by the firm that allow the authors to directly control for worker productivity. Despite the differences in scope between these firm-level data and the Swedish data, the results are qualitatively similar.

Liu et al. (2016) were the first to empirically look at whether cyclical skill mismatch is an important mechanism behind the persistent effects of recessions. They develop a measure for skill mismatch by looking at relative wage premia across college majors and industries: they define a mismatched graduate to be employed in an industry that does not value her college major (i.e. low wage premium).[23] They find skill mismatch to be counter-cyclical and an important factor in explaining the persistence of the negative effects on labor market outcomes: skill mismatch can explain as much as half of the effect on earnings.[24]

To conclude, the existing empirical evidence finds that recessions at labor market entry can have sizable and persistent negative effects on labor market outcomes. These findings are robust and have been documented in labor markets with various institutional features. Some studies also provide suggestive evidence that the negative effects are more persistent in countries with more rigid labor markets. Furthermore, in the more flexible North American labor markets, wages rather than employment seem to be the more relevant channel through which these effects materialize while the European evidence mostly suggests the opposite. Finally, a higher educational attainment seems to partly shield against adverse initial economic conditions: for more-educated individuals, the negative effects appear to be at least somewhat persistent, but not necessarily permanent, whereas much more sizable and even permanent scars have been documented for medium- and low-educated workers.[25]

[23] This measure has an obvious selection bias problem: those who find employment in the "right" industry during recessions might be positively selected based on ability and motivation, for example. In order to partially control for ability, the authors use IQ test scores from the Norwegian military records, but the results are unaffected.

[24] Liu et al. (2016) also use an alternative mismatch variable based on the worker flows of graduates with different majors to different industries. The results remain qualitatively similar.

[25] Obviously, some of these differences are attributable to endogenous selection to different kinds of education.

Chapter 3

Data

In this chapter, I first describe in Section 3.1 the data used for the econometric analyses. I also present the main outcome and explanatory variables I use. The empirical strategy used is presented and discussed in detail later in Chapter 5. In Section 3.2, I present in detail how I form the sample used in the analyses. The sample consists of Finnish university graduates who obtained a Master's degree between 1988 and 2004 and were aged 22–35 in the year of graduation.

3.1 Data & Main Variables

In performing the econometric analyses, I use individual-level microdata provided by Statistics Finland.[26] The data set I use is the Finnish Longitudinal Employer-Employee Data (FLEED). FLEED contains matched employer-employee panel data on all Finnish residents aged 15–70 for the period 1988–2014.[27] Unique encrypted personal identifiers allow following the same individuals over time. The data contain annual information on individuals' basic characteristics (age, gender, nationality, region of residence etc.), marital and socioeconomic status, family type and size, employment (e.g. number of months spent employed and unemployed), main activity during the whole year and in the last week of the year, educational attainment (year of completion for and type of the highest completed degree), and income (earned income, capital income, wage and salary income, received unemployment benefits, pensions etc.). The FLEED data also include some information on the individual's employer, including the type of ownership, legal form and industry of the enterprise. However, the data unfortunately do not contain

[26]The data set I use in this thesis has limited access. Access to the data can be obtained through an application process. For detailed information, see `https://tilastokeskus.fi/tup/mikroaineistot/index_en.html`.

[27]The data I have access to is the total FLEED data set. Statistics Finland also provides a 1/3 random sample of the data prepared for research use. See `http://stat.fi/tup/mikroaineistot/me_kuvaus_henkilo_en.pdf?_ga=1.135788659.353869278.1484721616`.

detailed information on the structure of earnings (wage rates, working hours etc.) or form of contract (full-/part-time employment).

The information contained in the FLEED data comes from the Employment statistics (*Työssäkäyntitilasto* in Finnish) published by Statistics Finland. The data for the Employment statistics are collected mainly from various administrative and statistical data files (around 40 data files in total). These include, for example, the register data files of the Tax Administration, the register of job applicants maintained by The Ministry of Economic Affairs and Employment (MEAE), and numerous register data files of Statistics Finland (e.g. the Register of Completed Education and Degrees, the Student Register, and the Register of Enterprises and Establishments). The reference period for the statistics is the last week of the year, but some of the information is collected throughout the statistical year (e.g. number of months employed/unemployed, annual earned income etc.). (Official Statistics of Finland, 2016b)

The FLEED data contain and allow constructing the main outcome variables concerning labor market outcomes. First, total annual real earned income (converted to 2012 euros using Statistics Finland's consumer price index data), henceforth simply "real annual earnings", measures the total annual wage and entrepreneurial income subject to state taxation. The second outcome variable is a dummy variable for being unemployed. Since the units of observation in the FLEED data are individual-year observations (instead of e.g. individual-level monthly/quarterly observations), there are many different ways to define this variable and it is not evident which of them should be preferred. In the main results in Section 6.2, I define a person as unemployed if her main activity during the last week of the year is being unemployed. Henceforth, I refer to this variable simply as "unemployment". This definition is sensible because the Employment statistics, from which FLEED is constructed, defines an individual as unemployed if she was registered as unemployed in the last working day of the year in MEAE's register of job applicants.[28] However, I also use two alternative definitions for the unemployment variable to show that my main empirical results are quite insensitive to how unemployment is defined (see Section 6.4). These alternatives define individuals as being unemployed if they are unemployed for at least one or three months during the year.[29] Finally, the third outcome variable is a dummy variable for whether the individual has received unemployment benefits during the year. Since the receipt of unemployment

[28] An individual is defined as unemployed in the register if she is not employed, not working full-time as an entrepreneur and is not a self-employed worker. An individual is also defined as unemployed if she is employed but fully laid off or her regular weekly working time is under four hours. (Official Statistics of Finland, 2016a)

[29] These alternative unemployment variables are constructed from the same variable that measures the number of months spent unemployed. A problem with this variable is that the way it is constructed changes during the considered time period. For the period 1988–2004, each month is considered *separately*: if the individual is unemployed for at least 16 days during the month, she is considered unemployed for the whole month. Starting from 2005, the number of months spent unemployed is calculated based on the number of days spent unemployed during *the whole year*. This clearly creates problems for the comparability of the values across years. This is another reason why I prefer the definition based on the main activity during the last week of the year.

benefits obviously correlates strongly with being unemployed, using this outcome variable provides another way of assessing the effects on unemployment. The conditions for receiving unemployment benefits are specified in Section 4.3.

To proxy for the economic conditions facing university graduates upon graduation, I use the unemployment rate in the year of graduation in the individual's area of residence as the main regressor in the empirical analyses. I compute these unemployment rates directly from the FLEED data because such data are not readily available. FLEED includes two options for the choice of geographical area: regions (*maakunta* in Finnish, 19 in total) and the larger major regions (*suuralue* in Finnish, 5 in total). These areas are equivalent to NUTS 3 and NUTS 2 areas, respectively, in the European Union's (EU) *Nomenclature of territorial units for statistics* (NUTS) classification system of statistical areas. FLEED contains the individual's annual region and major region of residence (in the last day of the calendar year) using a fixed year of reference (2014). Thus, the information concerning residence is not affected by past municipal consolidations and the computed unemployment rates reflect changes in the economic situation of the same fixed regions and major regions.

For the main results, I decided to use the regional unemployment rates for three reasons. First, the greater number of regions allows more variation in economic conditions facing graduates to be exploited in the analyses. Second, similar geographical areas have been used in the previous literature. For example, Liu et al. (2016) use Norwegian counties, which are also classified as NUTS 3 areas and are similar to Finnish regions in terms of population size, as the geographical areas in their analyses. Lastly, as will be discussed in Chapter 5, in the empirical analyses I work with grouped data, where the groups defined by year of graduation and area of residence in the year of graduation, and in estimations I use standard errors clustered at the group level. Using regions instead of major regions allows me to use a much larger number of groups and thus a larger number of clusters when computing standard errors. As Cameron et al. (2008), Cameron and Miller (2015, Section VI) and Angrist and Pischke (2009, Chapter 8) emphasize, having too few clusters in estimation when using grouped data may lead to downward-biased standard errors and thus unreliable statistical inference. Nevertheless, I show in Section 6.4 that using a major regional specification also yields similar results.

In Finland, two kinds of unemployment rates are published. The unemployment rates published by Statistics Finland are based on data from the sample-based Labor Force Survey (LFS), whereas the unemployment rates published by the Ministry of Economic Affairs and Employment in its Employment Service Statistics (*Työnvälitystilasto* in Finnish) are based on information from register data on job seekers registered with the Employment and Economic Development Offices (*TE-toimistot* in Finnish). Mainly because of differences in the definition of unemployment, the two unemployment rates

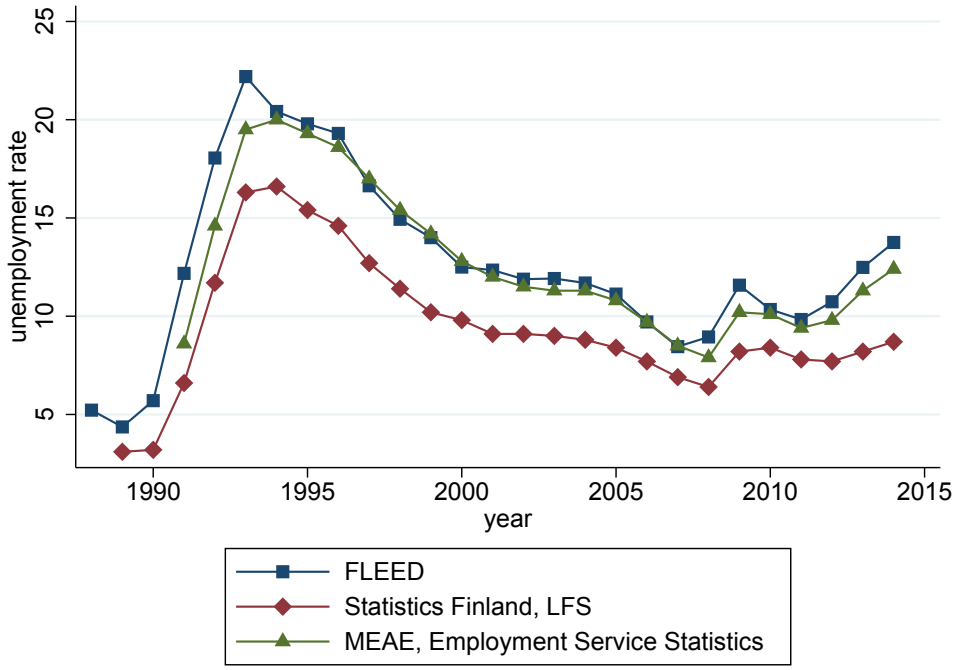
differ from each other, with MEAE's unemployment rates consistently being higher.[30] Another important difference worth mentioning in this context is the international comparability of the two unemployment rates. The definitions of unemployment and employment used by Statistics Finland when carrying out the Labor Force Survey follow the guidelines of the International Labour Organization (ILO) and the regulation of the EU, and thus the LFS unemployment rates are internationally comparable. In contrast, the MEAE unemployment rates are *not* internationally comparable because of the varying standards used by labor force administrations and differences in legislature on unemployment benefits in other countries. (Official Statistics of Finland, 2016a)

As the employment information in the Employment Statistics is based on the MEAE's Employment Service Statistics and FLEED is constructed using the information of the Employment Statistics, it seems reasonable to follow the Employment Statistics's definitions of employment and unemployment when computing the unemployment rates. Thus, I compute the regional unemployment rates from the FLEED data as follows: for each year, I identify all individuals aged 15–70 who belong to the workforce (i.e. the employed and unemployed) based on their main type of activity in the last week of the year. I drop all other individuals and aggregate the panel data to region-year cells and calculate the unemployment rate (i.e. $\frac{\text{no. of unemployed}}{\text{(no. of employed)+(no. of unemployed)}} \times 100\%$) in each cell. Thus the resulting figures measure unemployment rates in regions defined using the divisions valid in 2014. I also compute the major regional and national unemployment rates in a similar way. Note that because FLEED does not include individuals aged over 70, I am unable to compute unemployment rates for the population for which the main unemployment rates published by Statistics Finland and MEAE are calculated, i.e. individuals aged 15–74. Finally, I emphasize that, as discussed above, the resulting time series of unemployment rates and its descriptive statistics (mean, standard deviation etc.) cannot be compared internationally because the definitions of employment and unemployment can vary across countries.

To inspect how the unemployment rates I compute using FLEED compare with those published officially, I plot in Figure 1 the resulting national unemployment rates along with annual averages for Statistic Finland's LFS-based unemployment rates and the unemployment rates from MEAE's Employment Service Statistics (the publicly available time series for the LFS unemployment rates and MEAE's unemployment rates start from 1989 and 1991, respectively.) As can be seen, the unemployment rates computed from FLEED and MEAE's unemployment rates are consistently higher than Statistics Finland's LFS unemployment rates. As already discussed, this mainly reflects the differences between the definitions of unemployment between MEAE and Statistics Finland.

[30] In the LFS, a person is unemployed "if he/she is without work during the survey week, that is, has not done paid work or has not worked as self-employed, has sought work as an employee or self-employed in the past four weeks and could start work within two weeks." In the Employment Service Statistics, a person is unemployed if she is registered as a job seeker at the Employment and Economic Development Offices, and either is not employed or working full-time as an entrepreneur or a self-employed worker, or is employed but fully laid off or her regular weekly working time is under four hours. (Official Statistics of Finland, 2016a)

Figure 1: National Unemployment Rates, 1988–2014



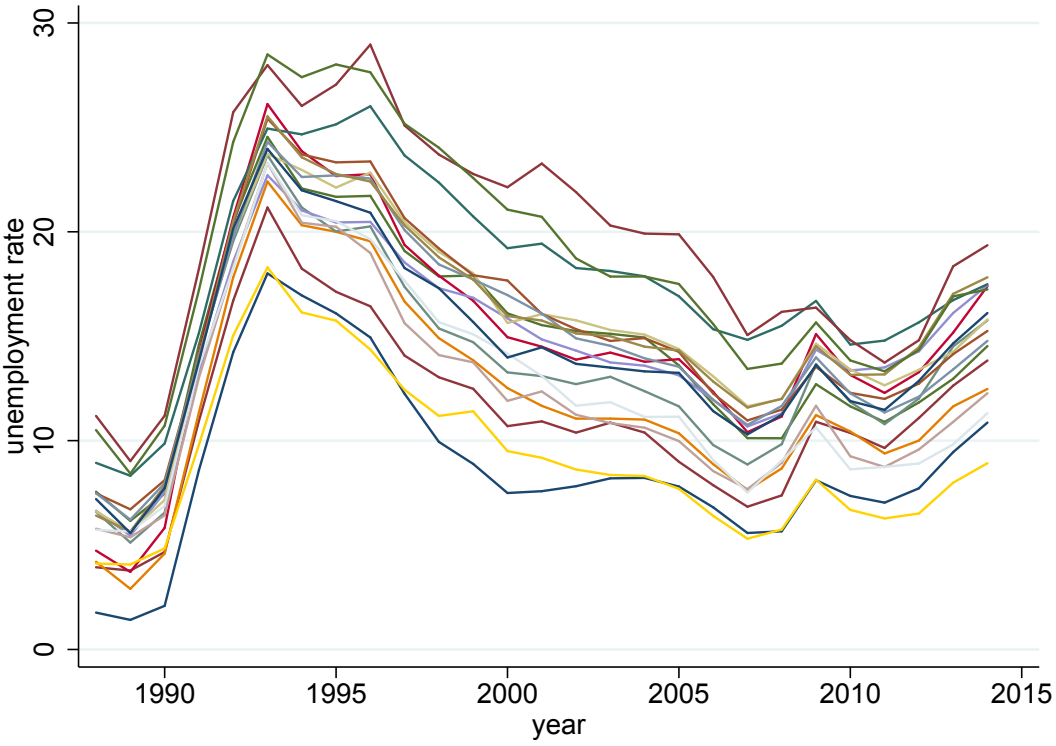
Notes: This figure contains the time series of national unemployment rates for the time period 1988–2014 computed using FLEED. See Section 3.1 for information on how these rates are computed. For the sake of comparison, the figure also includes the annual average unemployment rates published by Statistics Finland's Labour Force Survey and Ministry of Economic Affairs and Employment's Employment Service Statistics.

The unemployment rates computed from FLEED are for the most part close to the MEAE unemployment rates. This is encouraging since both unemployment rates use the same underlying definitions of unemployment. There are notable differences only for the years 1991–1993, i.e. during the deep depression, where the FLEED unemployment rates are roughly three percentage points higher. From there on the annual differences are at most 1.3 percentage points, with differences being well below a percentage point for most years. Encouragingly, all three unemployment rate time series evolve very similarly, thus capturing the business cycle variation in similar ways. One notable exception is the current recession: starting from 2010–2011, the FLEED and MEAE unemployment rates started to rise more quickly than the LFS unemployment rate which only saw a muted rise in comparison. All in all, this graphical comparison suggests that the FLEED unemployment rates I have computed and used for the analyses in this thesis are reliable, at least at the national level.

Finally, Figure 2 presents the time series of unemployment rates for 18 Finnish regions computed using FLEED.[31] As can be clearly seen, the unemployment rates evolve quite similarly over time in all regions, but there is considerable cross-sectional variation

[31]There are actually 19 regions, but I decided to omit the unemployment rates for Åland. This is because I drop all graduates for whom Åland is the region of residence in the year of graduation from the final sample. See Section 3.2.

Figure 2: Regional Unemployment Rates, 1988–2014



Notes: This figure contains the time series of unemployment rates for the time period 1988–2014 for all Finnish regions (excluding Åland). The unemployment rates are computed using FLEED. See Section 3.1 for information on how these rates are computed.

each year between regions. For example, during the depression of the early 1990s, the peak unemployment rate in some regions was under 20% whereas in others it was more than 25%. There is also clear idiosyncratic variation, over and above the common overall trend, in how the regional unemployment rate evolves over time. As discussed in Chapter 5, this idiosyncratic regional variation is used for the identification of the effects of facing adverse economic conditions upon graduation. Major regional unemployment rates are presented in Figure A1. Similarly to regional unemployment rates, a clear overall trend is easily discernible. Also idiosyncratic variation is present, albeit to a lesser extent.

3.2 Sample Formation

The main population of interest in this thesis is Finnish university graduates who received a Master's degree for the first time between 1988 and 2004 and were aged between 22 and 35 at the time of graduation. I limit my analysis only to university graduates for a couple of reasons. First, the majority of those obtaining a Master's degree enter the

labor market and start searching for a full-time job afterwards. This would likely not be the case if I also included those who obtained a bachelor's degree, since the majority of university students go on to obtain a Master's degree in Finland. Thus the year of obtaining a Master's degree offers a reliable and plausible timing of labor market entry. Of course, a subset of graduates opt to pursue doctoral studies. However, this is an interesting outcome to study in itself because facing a bad economy upon graduation potentially lowers the opportunity cost of further schooling. I briefly discuss this issue in Section 6.4. Second, highly-educated workers such as university graduates typically have strong labor market attachment. They are also more likely to make a career where they gradually progress to more advanced tasks. This means that for highly-educated workers, jobs early on in the career are important for accumulating appropriate kinds of human capital, as discussed in Section 2.1. Thus focusing on university graduates allows studying whether, and to what extent, poor economic conditions upon graduation disrupt their careers. Third, looking at university students allows identifying different groups of graduates by field of study, for example. This offers the possibility of studying the underlying heterogeneity of the susceptibility to adverse initial labor market conditions. While detailed analyses of this sort are unfortunately beyond the scope of this thesis, I hope that this thesis provides an impetus for these kinds of studies with Finnish data. Nevertheless, differences in fields of study may be relevant when I study the gender differences in the effects of adverse initial economic conditions in Section 6.3.

The level of the highest most recently completed degree is obtained from the code of qualification variable included in the FLEED data. The degrees are classified using the 6-digit *Finnish Standard Classification of Education 2010* system and are comparable across years.[32] The first digit in the code of qualification gives the level of the degree, while the second digit gives the field of the degree, allowing the possibility for comparing the effects of economic conditions upon graduation on graduates of different fields. The information about the degrees comes from Statistics Finland's *Register of Completed Education and Degrees*. A completed degree is included in the register only if the individual has a Finnish personal ID number. For this reason, the sample does not include e.g. many foreigners who have completed a Master's degree in a Finnish university but do not have a Finnish ID.

I form the main sample used in the analyses as follows. First, I identify the individuals who obtained a Master's degree for the first time between 1988 and 2004 and for whom age at graduation and the region of residence in the year of graduation are known. The time period I have chosen allows studying the first 10 years after graduation for all the cohorts included. Based on the evidence presented in Section 2.2 on the persistence of the negative effects found in the previous literature, looking at the first ten post-graduation years seems reasonable *ex ante*. In addition, the time period considered is interesting, especially because it includes the deep economic depression Finland experi-

[32] For a complete list of degree classifications, see `http://tilastokeskus.fi/meta/luokitukset/koulutus/001-2010/koko_luokitus_en.html`.

Table 2: Descriptive Statistics for the FLEED Sample

Variable	Mean	Std. Deviation	Observations
Male	0.446	0.497	1417740
Age in year of graduation	27.4	2.71	1417740
Year of graduation	1997	4.87	1417740
Unemployed	0.0361	0.187	1417740
Regional UR in year of graduation	12.9	5.94	1417740
Major regional UR in year of graduation	13.1	5.84	1417740
Real annual earnings (in 2012 euros)	39740	25586	1403609
Receives unemployment benefits	0.118	0.322	1417740

Notes: This table gives descriptive statistics for the individuals of the FLEED sample used in the analyses. See Section 3.2 for detailed information on how the sample is formed.

enced during the years 1990–1993 (see Section 4.4). To obtain the level of the completed degree, I use the first digit of the code of qualification, which is equal to 7 for Master's degrees.[33] After identifying these individuals, I drop all observations pre-dating the year of graduation and only keep those who were aged between 22 and 35 in the year of graduation, thus keeping approximately 89% of all individuals (roughly 154,000 individuals and 2,730,000 observations in total). Next, out of these individuals I only keep those individuals who are present in the data in each of the first ten years following the year of graduation. Fortunately, in doing so I end up dropping only roughly 12,000 of the individuals and 125,000 observations. Finally, I drop the graduates whose region of residence in the year of graduation is Åland because of the very low number of observations. After going through these steps, I am left with the final FLEED sample containing 141,774 individuals and 1,417,740 individual-year observations in the time period of interest, i.e. 1–10 years since graduation (and 2,602,090 observations overall).

Table 2 presents some descriptive statistics for the final FLEED sample. More detailed cross-sectional descriptive statistics concerning the main outcome variables for each of the first ten years after graduation are presented in Table A7. As can be seen in Table 2, the average age in the year of graduation is roughly 27 years, the average year of graduation is 1997, and roughly 45% of the graduates are males. The mean real annual earnings during the whole time period is roughly 39,700 euros. Mean annual earnings grow quite rapidly each year, though, starting from being around 29,400 euros in the year following graduation and ending up being over 49,000 euros by the tenth year (see Table A7). Each year, roughly 3.6% of the individuals are unemployed during the last week of the year and 11.8% receive unemployment benefits. Both the share of unemployed and of unemployment benefit receivers fall during the first ten years after graduation, starting from being 5.5% and 21.9% in the first year and ending up being 2.9% and 8.7% in the tenth year, respectively (see Table A7). Finally, we can see in Table 2 that there is considerable variation in the regional unemployment rate facing

[33]The first digit in the code is also equal to 7 for higher polytechnic degrees (*ylempi AMK-tutkinto* in Finnish). In order to focus on university graduates, I drop all individuals with these non-university degrees.

graduates upon graduation: the mean (and median) unemployment rate is roughly 13%, but ranges widely between 1.4% and 29% (not shown). Graduation cohort sizes by region and major region of residence in the year of graduation are presented in Tables A8 and A9, respectively. As can be seen in both tables, graduation cohort sizes at the national level show a general increasing trend over time. There is large regional variation in graduation cohort sizes, with Uusimaa having roughly 3000–4000 graduates each year while Central Ostrobothnia has less than 100 graduates in each year, for example. As I discuss in Section 6.4, the empirical results I obtain are not sensitive to excluding graduates from Uusimaa. At the major regional level, the cross-sectional variation is smaller.

Chapter 4

Institutional Setting

In this chapter, I describe the institutional setting and time period studied in this thesis. In particular, I discuss the relevant features of and changes in the Finnish tertiary education system (Section 4.1), labour market system and policies (Section 4.2), the unemployment insurance system (Section 4.3), and business cycle variation in Finland (Section 4.4). As already mentioned, the data used in this thesis covers the time period 1988–2014, and I look at the first ten post-graduation years of university graduates who obtained a Master's degree between the years 1988 and 2004.

4.1 Tertiary Education

In Finland, tertiary education is offered in universities and polytechnics (the latter are also called universities of applied sciences). Tertiary education is preceded by nine years of basic education in comprehensive school (normally ages 7–16) and then three years of upper secondary education either in a general upper secondary school (*lukio*) or a vocational upper secondary school (*ammattikoulu*). Both general and vocational secondary education provide eligibility to tertiary education. As a result of a degree reform that took effect in 2005, the majority of tertiary education programs were mandated to begin following a two-cycle structure that is compatible with the guidelines of the Bologna Process.[34] After the reform, university degrees in Finland gained their current structure with a lower university degree (usually awarding a bachelor's degree) of 180 credits and a planned duration of three years, and a higher university degree (usually awarding a Master's degree) of 120 credits and a planned duration of two years. Despite the target of completing both degrees within five years, the median study length

[34]The Bologna Process is an ongoing project launched in 1999 that currently has 47 participating European and other nearby countries. According to the Bologna Declaration (1999), it aims to e.g. create an easily readable and uniform degree system and promote the quality of European higher education. To facilitate achieving a uniform degree system, the process included the creation of a common system of study credits. See e.g. Niemelä et al. (2012).

in Finland has been 6 to 6.5 years for most years since 1987 (Häkkinen and Uusitalo 2003, Official Statistics of Finland 2004, 2014).

Because the degree reform came to effect in 2005, all the graduation cohorts included in the FLEED sample and used in the empirical analyses pre-date the reform. Fortunately, Finnish universities had already started to move towards the current degree system starting from the late 1980s, well before the reform. This tendency was motivated by attempts to make degrees more comparable internationally and to cut study times and dropout rates. Moreover, since 1994 universities were increasingly moving to the current two-cycle degree structure and the current three- and two-year completion times, with the exceptions of degrees in engineering and medicine. Thus, the degrees university graduates obtained during the time period of interest, 1988–2004, arguably were for the most part similar to those in the current system. Furthermore, the degree codes included in the FLEED data for the cohorts in the FLEED sample have been converted using the Finnish Standard Classification of Education 2010 system to match all degrees to the current system of degrees. (Niemelä et al., 2012)

University students were eligible for student benefits during the whole time period 1988–2004. Here, I highlight the major changes in the benefit system during this period and the motivations behind and implications of them. Before 1992, the student loan formed the majority of the benefits, with the student grant and the housing supplement accounting for roughly 15% of the total benefits. However, a comprehensive reform of the student benefit system in 1992 significantly increased the role of student grant, making it account for roughly two thirds of the total benefits. Student grant also became subject to taxation. Due to pressure from the Finnish banking sector, the reform also made student loans market-based and the former system of state-subsidized interest payments was abandoned. However, new student loans taken out after the reform became guaranteed by the government. After the reform, the demand for the student loan plummeted as the market-based real interest rates rose sharply: less than one fifth of university students took out the loan, compared to nearly half of the students before the reform. (Raivola et al. 2000; Häkkinen and Uusitalo 2003; Kela 2011, Appendix 7)

There were several reasons for the reform. These include e.g. the deregulation of the financial sector and the deep depression in the early 1990s. It was also noticed that the level of student benefits was lower than of other welfare benefits. Furthermore, the former student benefit system was seen to reduce equality in access to tertiary education, reduce study motivation, increase the probability of interrupting studies, and increase the length of studies (e.g. due to working while studying). Some aspects of the reform tried to incentivize full-time studying and graduating on time: for example, the maximum eligibility period for student benefits was reduced from seven years to 55 aid months (roughly equivalent to six years). (Raivola et al. 2000; Häkkinen and Uusitalo 2003; Kela 2011, Appendix 7)

Häkkinen and Uusitalo (2003) study the effects of the 1992 reform on the duration of studies. As already mentioned, one of the main arguments for the reform was to cut graduation times. They find that the reform only had a relatively small effect on graduation times, with the effects mainly seen in fields with long average durations of study. However, as the authors emphasize, it is hard to argue that these effects were mostly due to the reform. This is because the reform coincided with the 1990s depression which increased unemployment sharply: the small reductions in graduation times may have been in large part due to less opportunities available for working while studying.

The student benefit system saw only relatively minor changes after the 1992 reform. The student grant was reduced by roughly 10% and the housing supplement was reduced to account for 67% of the rent (from 75%) in 1995. In 1998, the earnings threshold used in determining the size of the student grant was changed from a monthly threshold to a calendar-year threshold. This change allowed students to work alongside studies more flexibly than before. After 1998, the student benefit system remained largely the same for the rest of the time period of interest (until 2004). It is worth noticing, though, that the real value of the student grant fell over time because it was not indexed. (Häkkinen and Uusitalo 2003; Kela 2011, Appendix 7)

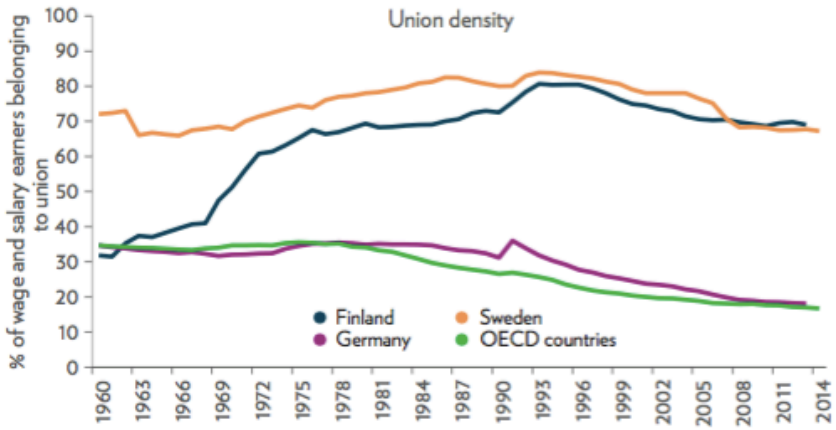
4.2 Labor Markets

Another important institutional feature of the Finnish economy for this thesis is the Finnish labor market system and its related legislature. An initial observation is that, according to the indicators of the Organisation for Economic Co-operation and Development (OECD) which measure the regulation on dismissals of workers with permanent and temporary contracts, the strictness of the employment protection legislation (EPL) in Finland is below the OECD mean. However, the Finnish EPL is clearly stricter than in the U.S. and Canada, for example. Compared to similar economies (e.g. Sweden, Germany), the regulation of permanent workers is more lenient whereas the regulation of temporary workers is stricter. However, the Finnish EPL has less exemptions for small firms in comparison to other OECD countries with similar EPL strictness levels. For example, in Finland firms employing less than 20 workers (which employ roughly a quarter of all workers) are not compelled to hold consultations with employees prior to firings. (Böckerman et al., 2017, pp. 8–9 and Figure 1)

Similarly to other Nordic countries, wages are set by collective labor bargainings in Finland for the vast majority of workers.[35] While no statutory minimum wage exists, collective bargains result in a wide range of industry-specific minimum wages and specific

[35] Good overviews of the Finnish collective wage bargaining system are provided by Asplund (2007) and Sauramo (2012), for example.

Figure 3: Unionization in Finland, selected other countries, and in OECD countries. (Source: Böckerman et al. 2017, p. 13.)



wage levels that can depend on individual characteristics (e.g. educational attainment, experience, skills, job difficulty and location). By default, collective bargains are applied to the members of the negotiating parties (union members and employers). However, if the fraction of workers who belong to the unions participating in the negotiations is representative enough, the bargain can be extended to cover all workers in the relevant sector. Given that the union participation rate in Finland is high (having varied roughly between 70% and 80% since 1988, similarly with Sweden), this means that around 90% of all workers are covered by a collective bargain in Finland (see Figure 3 and Böckerman et al. 2017, pp. 11–12).

What is important to know is to what extent collective bargains in reality bind wages and cause real or nominal wage rigidity. Böckerman et al. (2017) find that collectively agreed wages appear to be somewhat binding, at least in some primarily low-wage sectors. Böckerman et al. (2010) study wage rigidity for the time period 1985–2001 with payroll record data on private sector workers provided by the employers' associations. They conclude that in Finland changes in real wages in response to business cycle fluctuation happens primarily at the macroeconomic level (average wages adjust) by means of collective agreements, while there are only small adjustments at the microeconomic (individual) level. Moreover, real average wages can be allowed to adjust downwards in recessions, but nominal wage cuts are resisted by labor unions even under adverse economic conditions. Complementing the above discussion, Vainiomäki (2016) looks at the evolution of wage dispersion and wage rigidity in Finland for the time period 1995–2013. He notes that wage dispersion increased until the economic crisis which started in 2008, after which dispersion has not for the most part increased. When studying the sources of the wage dispersion, Vainiomäki (2016) finds that the within-firms variance in wages

is larger than the between-firms variation.[36] Furthermore, both variance measures have increased, but not similarly. After the 1990s economic depression, the increase in the wage dispersion happened more between firms, meanwhile the within-firm variance has played a larger part in the 2000s. The larger role of within-firm variance would suggest that potential earnings losses from graduating into a recession could come from e.g. task downgrading.

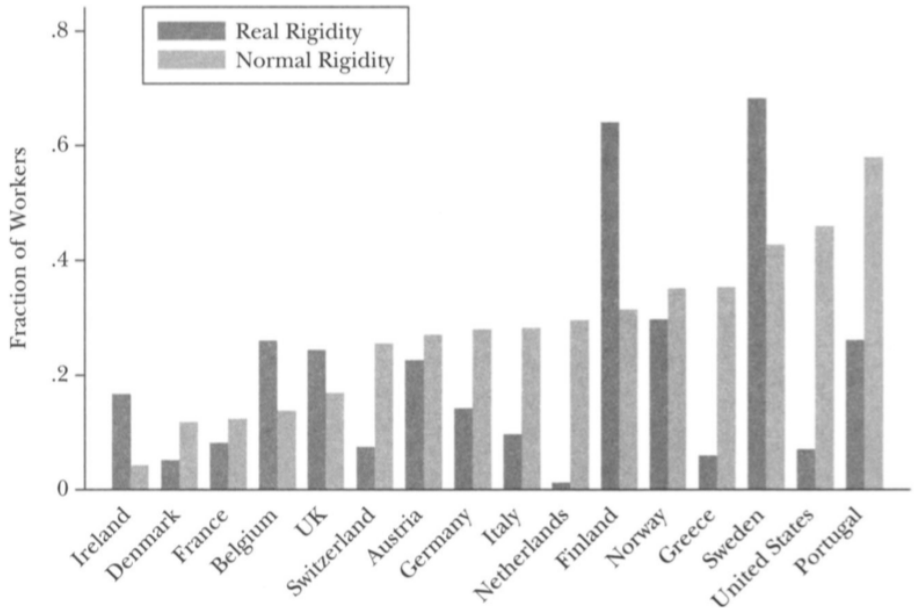
Studying the time period 1996–2010, Sauramo (2012) finds that the collectively agreed wage increases have been similar across different sectors (private and public sectors), which reflects the high level of centralization among and co-ordination between negotiating parties in collective bargaining. Furthermore, in the whole economy wage drifts (i.e. local-level deviations from the collectively agreed bargains) have in general roughly accounted for a third of the increases in average wages. Differences between sectors and industries exist, however. For example, wage drift has normally been more volatile and pro-cyclical in the private sector. In addition, wage drift has fluctuated more within industries where performance-related supplements and other variable forms of pay account for a larger fraction of earnings (e.g. in financial intermediation). As Sauramo (2012, p. 16) notes, performance-related pays are especially relevant for white-collar workers who normally are highly educated. This suggests that pro-cyclical fluctuations in wage drift may cause earnings losses for university graduates who graduate and enter the labor market upon a recession, especially if they work in the private sector.

From an international perspective, empirical evidence indicates that real wages are more rigid in Finland than in many other countries. When comparing real wage rigidity to countries for which there are studies relevant to this thesis, estimates from the International Wage Flexibility Project (IWFP) indicate that real wages are considerably more rigid in Finland than in Belgium, the United States, and Norway (see Figure 4). While the time period used for the calculations in Figure 4 ends roughly in the early 2000s/late 1990s (depending on the country), Vainiomäki (2016) notes that real wage rigidity has been relatively stable in Finland since the mid-1980s with an average rigidity estimate of 0.8 (i.e. 80% of workers who can be affected by downwards-rigid real wages have been affected by it).

Böckerman et al. (2017) conduct a literature review on the employment effects of a strict EPL and collectively bargained wages. They conclude that it is hard to find credible and clear empirical evidence indicating that a stricter EPL or more extensively bargained wages would cause sizable negative employment effects. However, they do find more visible negative effects on labor turnover and job creation. This could imply that in countries with stricter EPL and more binding wage levels, the hiring rate of graduates could remain low for a longer time in the case of adverse economic shocks,

[36] An increasing between-firms wage variation can reflect increasing productivity differences between firms or increasing matching of high-productivity workers to firms paying high wages, for example. On the other hand, increasing within-firm wage variance can reflect increasing variation in tasks or employee characteristics within firms, for instance. (Vainiomäki, 2016, pp. 7–8.)

Figure 4: Real and Nominal Wage Rigidity in Selected Countries. (Source: Dickens et al., 2007)



Notes: This figure reproduces Figure 3 of Dickens et al. (2007). It shows the fraction of workers in each country who are potentially affected by downward real and nominal wage rigidity.

suggesting that unemployment could be an important mechanism through which adverse labor market effects materialize. As noted in Section 2.2, some studies support this hypothesis, including Liu et al. (2016), Cockx and Ghirelli (2016), and Genda et al. (2010).

To conclude this subsection, the Finnish labor market system can overall be described as having a level of employment protection that is close to OECD average (but stricter than in Canada and the U.S.), but the extensive collective bargaining system causes real wages to be more rigid than in many other countries. However, the existing literature does not provide a clear-cut link between (un)employment effects, EPL, and the rate of co-ordination in wage setting and EPL. Nevertheless, the observed real wage rigidity in Finland does not rule out potential negative earnings effects, since they can arise also due to e.g. task down-grading.

4.3 Unemployment Insurance

Next, I provide a brief overview of the Finnish unemployment insurance (UI) system.[37] In Finland, eligibility for unemployment benefits requires that the individual is reg-

[37]This section draws especially from Kyyrä et al. (2017), who provide an excellent overview of the system and discuss the changes in it since the year 2000. For more extensive analyses the Finnish UI system, see Section 2 of the said report as well as Uusitalo (2005).

istered as an unemployed job seeker in an Employment and Economic Development Office (*TE-toimisto* in Finnish), is searching for a full-time job and ready to accept a potential job offer. The individual also has to form an activation plan that may entail participation in different forms of active labor market policies (ALMP). In contrast to many other European countries (see e.g. Esser et al. 2013), in Finland UI is provided through a *voluntary state-subsidized system* where benefits are paid out by unemployment insurance funds that are mostly administered by labor unions. However, the state subsidizes and supervises the system, but also regulates it by e.g. mandating that UI must be provided in the same manner by all funds. Roughly 90% of all employed persons belonged to a UI fund in 2015. (Kyyrä et al., 2017, Section 2)

The UI funds pay earnings-related benefits provided that the individual satisfies the employment condition, which currently requires having worked and contributed payments to the fund in at least 26 weeks (called "contribution weeks") of the last 28 months (called "review period"). Currently, UI benefits can be received for at most 400 (300) days if the worker has at least (less than) 3 years of work history. If calendar weeks are instead used, regular UI benefits can be received for at most 60, 80 or 100 weeks depending on work history and age. The maximum of 100 weeks is similar to the duration in Denmark, but longer than in e.g. Sweden and Germany (Esser et al., 2013). The benefit amount is determined by the average wage rate during the time period considered for satisfying the employment condition. The replacement rate is relatively progressive but there is no ceiling for the benefit amount. Individuals can also receive partial UI benefits during involuntary part-time employment or short-term full-time employment. UI fund members who do not satisfy the employment condition (or have used up the earnings-related benefits) are eligible for a means-tested flat-rate labor market subsidy that is available indefinitely. Non-members satisfying the employment condition can receive a flat-rate but non-means-tested basic unemployment allowance for the same time period used in the payment of earnings-related benefits. (Uusitalo 2005; Kyyrä et al. 2017, Section 2)

Uusitalo (2005) provides an overview of changes in the Finnish UI system for the time period 1984–2005. He identifies two broad periods during which the UI system evolved in opposing directions in terms of its generosity. Between 1984 and 1990s depression (see Section 4.4), the system became more generous: for example, the former system of downwards-graduated replacement rate on earnings-related benefits was abandoned in 1989, basic unemployment allowance increased according the evolution of the mean wage rate, and the entitlement period was lengthened. In contrast, after the 1990s depression and roughly until 2003, the system became more stringent due to the stricter eligibility conditions and reductions in benefit levels, for example.

Kyyrä et al. (2017, Section 2) summarize the changes in the UI system since the year 2000 (until January 2017) and evaluate their effects. Since 2003, they note that changes in the UI system have made the employment condition less stringent, especially for

those claiming benefits for the first time (e.g. through reductions in required contribution weeks), altogether making the employment condition closer to the situation in the early 1990s. In contrast, changes to the entitlement period have made the system less generous. Moreover, the overall UI benefit levels became more generous until 2012. When assessing the overall effect of these changes for individuals entering a new spell of unemployment, they conclude that UI benefits became more generous during the time period 2002–2014. Benefit generosity increased on average for workers with 3–19 years of work history while mirroring the situation in the early 2000s for those with less than three years of work history. However, the authors note that younger unemployed individuals are under-represented in their calculations; thus, the validity of these results to e.g. recent university graduates has to be taken with caution.

To conclude this subsection, it is instructive to place the Finnish UI system into an international context. In comparison to other European countries (using the systems in place in 2010), the analyses of Esser et al. (2013) indicate that the Finnish UI system has net replacement rates for both regular UI benefits and UI assistance that are close to those of e.g. Germany, Sweden and Denmark and benefit duration mirroring that of Denmark (but longer than of Germany and Sweden). Furthermore, the number of contribution weeks (ignoring the length of the reference period) needed for UI entitlement is roughly similar in Sweden (but less than in Germany and Denmark).

4.4 Business Cycle Variation

The variation in economic conditions during the time period of interest in this thesis (1988–2014) has been quite sizable in comparison to countries that have been previously studied in the literature relevant to this thesis (e.g. Norway, Belgium, the U.S., and Canada, see Section 2.2). In this subsection I quickly summarize the business cycle variation in the Finnish economy.

After the economic boom of the late 1980s, Finland experienced in the beginning of the 1990s what Gorodnichenko et al. (2012) call "the deepest economic contraction in an industrialized country since the 1930s and the deepest recorded peace-time recession in Finnish history". The devastating effects of the depression on the labor market and the macroeconomy as a whole, as well as its intergenerational effects have been widely studied (for a comprehensive study, see e.g. Kiander 2001). For example, real GDP fell by 11%, real consumption by 10%, and investment levels were at worst only 55% of their levels in 1990 (Gorodnichenko et al., 2012). As can be seen in Figure 1, the unemployment rate (using the LFS rates of Statistics Finland) rose dramatically, more than quadrupling from less than 4% before the crisis to more than 16% in 1993. The number of long-term unemployed (unemployed for over a year) rose from roughly 3,000 in 1990 to roughly 140,000 in 1995 (Kiander, 2001, p. 82).

There have also been some studies on the effects on students who graduated during the depression. For example, in a descriptive study Loukkola (2012) compares those who obtained at least an upper secondary degree in 1992 and in 2002. She shows that those who graduated in 1992 (and did not obtain a higher degree afterwards) faced considerable difficulties in finding employment: 10.5% of them were unemployed five years after graduation and 7% were unemployed after ten years after graduation. The corresponding figures for those who graduated in 2002 are 8.5% and 4%, respectively. However, a higher educational attainment appears to help considerably. For example, the author doesn't find any difference in the share of those who found employment five or ten years after graduation when comparing those who obtained a Master's degree: among both graduation cohorts, 87% had found employment five years after graduation and 90% ten years after graduation.

While the 1990s depression was remarkably deep, the following upturn began quite quickly. This change was particularly driven by the export sector that benefited from the quickly devaluing Finnish mark after the government decided to float the currency (Kiander, 2001, Chapter 6). The unemployment rate began falling steadily after 1993–1994 (see Figure 1). After a long period of stable economic conditions and falling unemployment, Finland also faced a recession in 2008 due to the global financial crisis. The effects of the crisis were larger than in many other countries: e.g. real GDP fell by 8.3% in 2009 according to Statistics Finland. Unemployment rate also started to increase and continued to increase persistently after a short decline in 2010–2011 (see Figure 1).

Chapter 5

Methodology

In this chapter, I first present the empirical strategy used in this thesis in Section 5.1. The methodology is very similar to what has been used in the previous literature (see e.g. Oreopoulos et al. 2012, and Liu et al. 2016) and takes advantage of the possibilities provided by matched employer-employee panel data. In Section 5.2, I discuss the potential problems of the chosen strategy and provide ways to address them.

5.1 Empirical Strategy

As has already been discussed, the aim of this thesis is to estimate the magnitude and persistence on labor market outcomes of the effects of facing adverse economic conditions upon graduation for the population of Finnish university graduates who obtained a Master's degree. As a proxy for the local economic conditions upon graduation, I use the regional unemployment rate in the year of graduation in the individual's region of residence.[38] However, there are numerous observable and unobservable characteristics that are correlated with the unemployment rate in the year of graduation and that can affect the labor market outcomes of graduates. Thus omitted variable bias poses a problem for reliable causal inference.

Fortunately, the panel structure of the matched employer-employee data helps in addressing the omitted variable bias by allowing to control for a number of observable and unobservable characteristics that are either cross-sectionally or longitudinally constant. Including regional fixed effects allows controlling for time-invariant region-specific characteristics that are correlated with both labor market outcomes and the unemployment rate upon graduation; these include e.g. various geographical, sociocultural and eco-

[38]Some studies in the literature (see e.g. Liu et al. 2016) use the unemployment rate of the region of the *college/university* from which the individual has graduated. Unfortunately, I do not have information on the location of the university.

nomic characteristics of the region that stay constant over time. Time effects, on the other hand, allow controlling for time-varying characteristics commonly shared and felt by *all* regions that are correlated with both labor market outcomes and unemployment rates; these can include various common national trends in economic and sociocultural activity. More importantly, time effects capture the component of business cycle variation that is common to all regions. This implies that the variation in regional unemployment rates used for the identification of the effects on labor market outcomes is the residual idiosyncratic region-specific variation in unemployment rates, over and above the variation commonly shared with all other regions.

I now turn to present the empirical strategy I use. To start, note that the panel data I have are grouped (or clustered): the data consist of annual observations on individuals who belong to groups defined by graduation cohort (year of graduation), denoted by c, and region of residence in the year of graduation, denoted by r. Because the main regressor of interest used in the analyses, the unemployment rate in the region of residence in the year of graduation, varies only at the cr-group level (rather than at the individual-level), it is a *fixed* regressor within the cr-group. This means that individual-level outcomes within each group are positively correlated. Thus individual-level observations within groups cannot be seen as independent: for example, a recession in the region of residence upon graduation lowers the earnings of many of the graduates from that region. As illustrated by e.g. Moulton (1990) and Angrist and Pischke (2009, pp. 308–315), if this within-group correlation is not taken into account, the usual heteroskedasticity-consistent standard errors (or the conventional OLS standard errors assuming homoskedasticity) can be severely biased downwards, even with the presence of a small within-group correlation between observations. Furthermore, the individual-level observations are also very likely to exhibit serial correlation (i.e. correlation across years): for example, an individual's employment status in a given year is likely to be correlated with her employment status in previous years.

How can we deal with both within-group correlation and serial correlation? First, to acknowledge the within-group correlation between individuals in any given year, I aggregate the individual-level microdata to cr-groups and work with grouped panel data where observations are annual group-specific means of the outcome variables (i.e. crt-observations, where t denotes the calendar year). This approach is suggested by e.g. Angrist and Pischke (2009, pp. 312–313) (in a cross-sectional context) and also suggested by Bertrand et al. (2004) and Cameron and Miller (2015). This choice also follows the existing literature (see e.g. Oreopoulos et al. 2012 and Liu et al. 2016). Furthermore, my choice of using group-level data instead of individual-level microdata partly reflects the fact that I do not have information on many potentially important individual-level control variables (e.g. background information on parents).[39] Therefore, I see the use of grouped data as a more conservative approach in this case. Since

[39]Some studies work with individual-level data and also include a vector of control variables for individual-level background characteristics.

the observations are group-specific means, I weight the group-specific observations by the corresponding group sizes (i.e. the number of individuals in a *cr*-group). Second, because individual-level observations within the groups are likely to exhibit serial correlation, also the group-specific means of outcome variables are likely to be serially correlated. To deal with this issue, I cluster the regression standard errors from the grouped panel data estimations at the group (*cr*) level, i.e. by graduation cohort (year of graduation) and region of residence in the year of graduation.[40] As noted by Angrist and Pischke (2009, pp. 318–319), this is the simplest and most widely used way of addressing serial correlation in studies using group-structured panel data, and is incidentally also the approach followed in many previous studies relevant to this thesis (see e.g. Oreopoulos et al. 2012, and Liu et al. 2016).

I can now proceed to detail the econometric model used in the analyses. The main econometric model to be estimated is given by

$$\bar{y}_{crt} = \alpha + \sum_{e=1}^{10} \beta_e UR_{cr0} + \phi_t + \theta_r + \gamma_e + \chi_c + \theta_r \times \gamma_e + u_{crt}, \quad (1)$$

where

- α is the constant term,
- $\bar{y}_{crt}$ is the group-specific mean of the outcome variable (logarithmic real annual earnings, unemployment dummy, dummy for receiving unemployment benefits etc.) of graduation cohort c from region r in calendar year t,
- UR_{cr0} is the regional unemployment rate facing regional graduation cohort cr in the year of graduation,
- β_e is the coefficient of interest denoting the effect of regional unemployment rate in the year of graduation on the outcome variable $\bar{y}_{crt}$ in potential experience year e $(\in \{1, \ldots, 10\})$,
- ϕ_t denotes the calendar year effects (i.e. time effects),
- θ_r denotes fixed effects of the region of residence in the year of graduation,
- γ_e denotes the potential work experience (years since the year of graduation) fixed effects,
- χ_c denotes the graduation cohort fixed effects, and
- u_{crt} is the error term.

Here, the graduation cohort fixed effects χ_c capture time-invariant differences in the characteristics of different graduation cohorts at the *national* level. In addition, potential work experience fixed effects γ_e capture the common labor market experiences

[40] I emphasize that I use one-way clustering at the *cr*-level, *not* multiway clustering on both *c* and *r*.

of all individuals with the same amount of potential work experience. Following Liu et al. (2016, p. 5), I also include the interaction term $\theta_r \times \gamma_e$ to allow the experiences of individuals with the same amount of potential experience to vary by region of residence in the year of graduation. To be able to include a separate constant term, α, in the model, I omit one of the graduation cohort dummies from the model to avoid problems with perfect multicollinearity and to be able to identify calendar year effects, graduation cohort fixed effects, and potential work experience fixed effects separately (see Oreopoulos et al., 2012, p. 7, footnote 10). As already discussed, standard errors are clustered at the cohort–region (cr) level in order to allow serial correlation within each region-cohort group (see Oreopoulos et al. 2012, p. 7, and Liu et al. 2016, p. 5). Because the observations are group-specific means of the outcome variable, I estimate the model with Weighted Least Squares (WLS) using group sizes as weights. As pointed out by Angrist and Pischke (2009, pp. 40–41, 312–314), this produces point estimates that are identical with estimates from a regression with the same set of regressors as in Equation 1 but using individual-level observations for the outcome variables instead.

If the main identifying assumptions are satisfied (see Section 5.2), the coefficient of interest in Equation 1, β_e, can be interpreted as the causal effect on the labor market outcome variable in potential experience year e of a percentage point increase in the region-specific unemployment rate in the year of graduation. This coefficient is allowed to variate by years of potential experience in order to assess the persistence of the effect of the initial regional unemployment rate facing the graduation cohort. In other words, we more specifically have $\sum_{e=1}^{10} \beta_e UR_{cr0} = \sum_{j=1}^{10} \beta_j [UR_{cr0} \cdot 1(e = j)]$, where $1(e = j)$ is an indicator function equal to one when potential work experience equals $j \in \{1, \ldots, 10\}$. Since potential work experience fixed effects, γ_e, and the interaction term $\theta_r \times \gamma_e$ are included, the causal effects measure deviations from the career trajectory of a graduate from the same region who faces more normal regional economic conditions upon graduation. Finally, $e \in \{1, \ldots, 10\}$ because I limit myself to only looking at the first ten years after the year of graduation, as already discussed in Section 3.2.

I use *potential* years of work experience (i.e. years since the year of graduation) instead of *actual* work experience because of the endogeneity of actual work experience; for example, graduates who are unemployed at some point during the first 10 years since graduation can be different from those who are always employed in unobserved ways. The data set I use does not have information on actual work experience, either. Finally, I begin to look at the effects from the first year after graduation since I do not observe the month of graduation for the whole time period. Not having data on the month of graduation could thus lead to misleading comparisons: for example, I could compare the earnings of those graduating at the beginning of the year and starting to work to those graduating in May and starting to work.

5.2 Threats to Validity

The main identifying assumption that needs to be satisfied in order to be able to interpret the coefficients of interest in Equation 1, β_e, causally is that the error term u_{crt} be uncorrelated with the regional unemployment rate in the year of graduation, UR_{cr0}. In other words, this means that the changes in the regional unemployment rates arise from changes in aggregate labor demand that are not correlated with graduation cohort characteristics. Obvious threats to the validity of this assumption are selective timing and region of graduation, which make the composition of graduation cohorts in a given region correlated with the unemployment rate in the year of graduation. Simply put, if a significant fraction of students strategically postpone their graduation or move between regions in response to adverse shocks in the local labor market, the set of coefficients (β_e) are subject to selection bias. Hypothetically, in adverse economic conditions individuals who are and feel more motivated and competent are more likely to enter the labor market. In addition, there is evidence that more educated individuals are more likely to move in response to changes in the local and more distant labor market conditions (see e.g. Wozniak, 2010). These effects would imply that the estimated effects on labor market outcomes are likely to provide lower bounds of the true effects. These problems can be partly dealt with by including the graduation cohort fixed effects, χ_c, which capture differences between graduation cohorts at the *national* level. However, if there is considerable variation between regions on how changes in the unemployment rate affect graduation cohort composition, the coefficients of interest β_e can still be biased.

Another potential problem is selective place of graduation. Since June 1994, when the Municipality of Residence Act[41] (201/1994) came into effect, university students have been able to register their migrations to the place of study as permanent moves. A natural concern is that this could lead to selective migrations to regions with e.g. better employment opportunities upon graduation. A related problem comes from the fact that the last day of the year is used as the reference point when defining the region of residence. If an individual graduates before that (e.g. in May), she can move to another region during the same year, perhaps in response to adverse economic conditions in the region from which she graduated. Since these possibilities would make the region of residence correlated with regional unemployment rate in the year of graduation, selective place of graduation would introduce selection bias to the estimates of interest, β_e. If graduates strategically choose the region of residence with the best employment prospects, selective choice of region would bias the estimates downwards. Thus, if both time and place of graduation are correlated with local labor market conditions upon graduation, the set of estimates β_e are likely to provide the lower bound of the true effects of adverse economic conditions upon graduation, as discussed.

[41] *Kotikuntalaki* in Finnish.

The problems of selective place and timing of graduation have been acknowledged in several ways in the literature. For example, Kahn (2010) uses an instrumental variable (IV) strategy by instrumenting state unemployment rate upon graduation with the unemployment rate in the state where the individual lived at age 14. Consistently with the hypothesis that the estimates of interest from the fixed effects model are biased downwards, her IV estimates are larger in magnitude. Some studies have observable data on the duration of education (see e.g. Oreopoulos et al. 2012, and Liu et al. 2016). In these studies, selective timing of graduation has been acknowledged by instrumenting for the unemployment rate in the year of graduation with the unemployment rate in the *predicted* year of graduation.[42]

Unfortunately, I do not have data on the duration of education. However, as a robustness check I inspect in Section 6.4 whether the regional unemployment rate is correlated with the size of the regional graduation cohort. To do this, estimate a model where the logarithm of the regional graduation cohort size is regressed against the regional unemployment rate, regional fixed effects, and year of graduation effects or a (linear/quadratic) year of graduation trend. Accounting for the year of graduation effects/trend should allow me to see whether changes in business cycle conditions produce changes in regional graduation cohort sizes, over and above the general trend in graduation cohort size fluctuation at the national level. If no such correlation is observed (or the correlation is only limited), it would suggest that selective timing of graduation is not a serious threat to the validity of the empirical analyses. It is also hard to assess whether selective place of graduation poses a problem since I do not have information on the region of the university in which the graduate was enrolled. However, it is arguably likely that the region of residence in the year *before* the year of graduation serves as at least a decent approximation of the region of the university. Therefore, as a robustness check in Section 6.4 I investigate whether there is a positive correlation between the regional unemployment rate in the year of graduation in the region of residence of the year before graduation and the probability that the region of residence in the year of graduation and the year before graduation are different. If there is no positive correlation (or only a small one), it would suggest that selective place of graduation is not a concern in my setting.

[42] This instrument works since these studies usually only include in their samples those who graduated in or after the target time, i.e. not faster than predicted.

Chapter 6

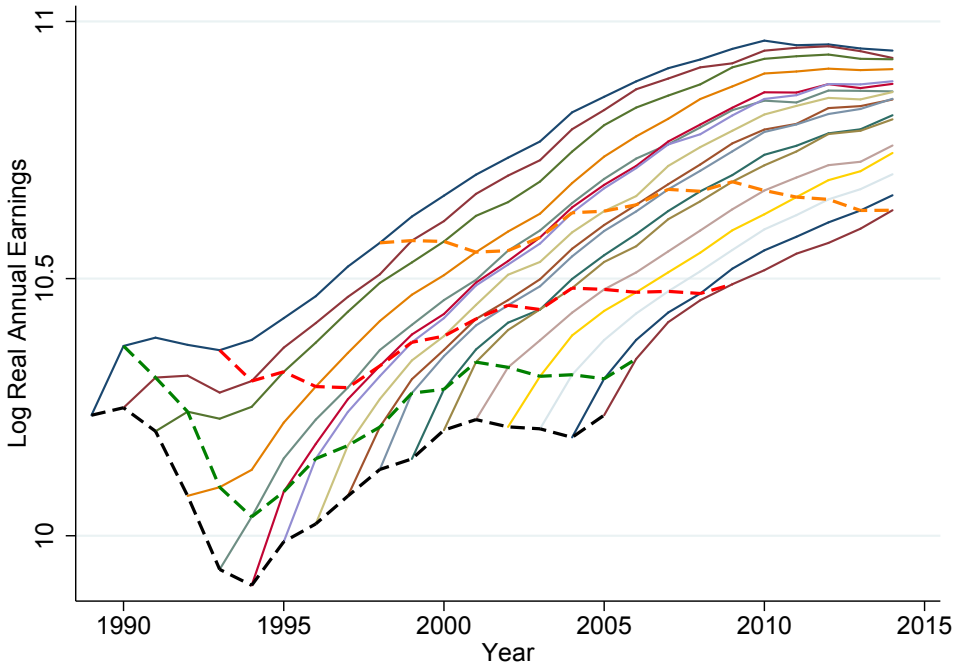
Results

In this chapter, I present the empirical results of the thesis. First, in Section 6.1 I present graphical descriptive evidence concerning the development of mean real annual earnings and incidence of unemployment over time among different graduation cohorts at the national level. After that, in Section 6.2 I proceed to present the main regression results concerning the effect of regional economic conditions upon graduation on real annual earnings, unemployment and receipt of unemployment benefits using the FLEED sample. In Sections 6.3 and 6.4, I complement the main results by providing various heterogeneity analyses and assess the sensitivity and robustness of the results. Finally, in Section 6.5 I sum up the empirical findings and relate them to the existing literature.

6.1 Descriptive Evidence

Before presenting the regression results, it is instructive to first provide descriptive information on how the labor market experiences of different graduation cohorts differ. Following the existing literature (see e.g. Oreopoulos et al. 2012, Liu et al. 2016, and Brunner and Kuhn 2014), I provide graphical evidence on the effects of economic conditions upon graduation on labor market outcomes. Figures 5 and 6 present the evolution of mean logarithmic real annual earnings and mean unemployment for the graduation cohorts 1988–2004 at the national level using the FLEED sample. The figures have been constructed by first aggregating the individual-level panel data to groups defined by graduation cohort (year of graduation) and calendar year and computing the group-specific mean logarithmic real annual earnings and share of unemployed for each year following the year of graduation. Thus, each solid curve represents the experience profile of mean real annual earnings or mean unemployment for a single graduation cohort over time. Furthermore, to compare different graduation cohorts at the same stage in their careers, the dashed lines connect the earnings or unemployment experiences of different

Figure 5: Earnings Profiles by Graduation Cohort



Notes: This figure presents the evolution of mean logarithmic real annual earnings for graduation cohorts 1988–2004 at the national level. Dashed lines connect the mean logarithmic real annual earnings of all cohorts at one (black), two (green), five (red) and ten (orange) years of potential experience. Earnings measure the total annual earned income, i.e. the sum of wage and entrepreneurial income subject to state taxation. Computed using the FLEED sample. See Section 6.1 for more discussion.

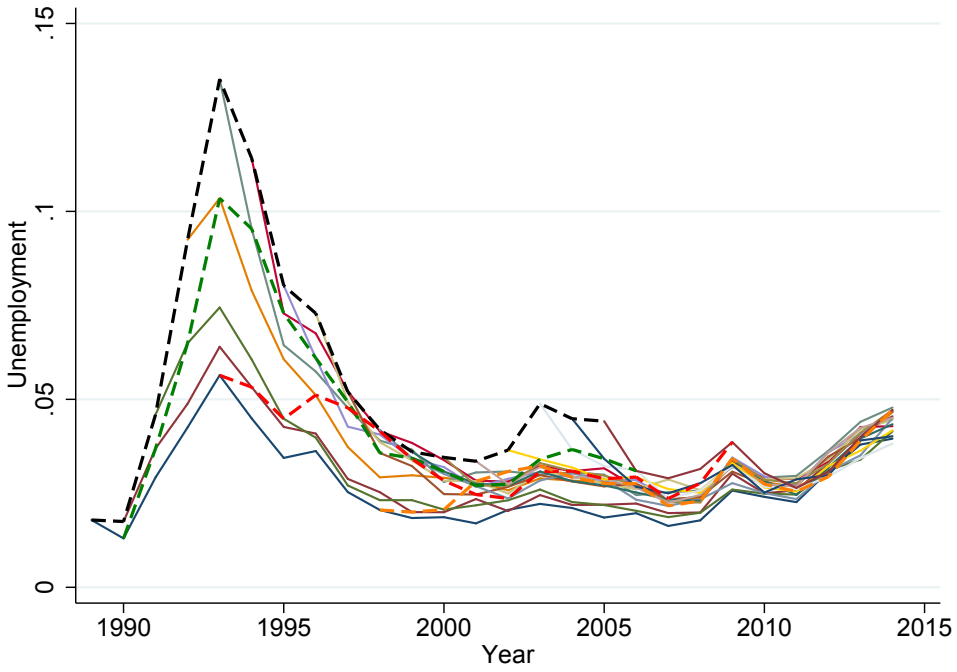
cohorts with the same amount of potential work experience.[43]

It is evident from Figure 5 that there are sizable differences between cohorts in the evolution of mean earnings.[44] The variation across cohorts in real annual earnings in the first year after graduation (black dashed line) is sizable and clearly coincides with the business cycle variation in Finland. For example, the real annual earnings one year after graduation were roughly 31,700 euros for cohorts who graduated just before the 1990s depression (cohorts 1988 and 1989), whereas for cohorts graduating during the depression (in 1992 and 1993) they were roughly 24,000 euros, approximately 25% lower. The initial mean annual earnings started to rise after the depression ended and the subsequent economic upturn started. It is notable, however, that only for the cohorts who graduated in 1999 and later were the mean real annual earnings in the year

[43] As explained in Section 3.2, each individual in the FLEED sample appears in the data in each of the first ten years after the year of graduation. Thus the cohort-specific means in Figures 5 and 6 are always computed using the same individuals for this time period. However, after the first ten years following graduation, some individuals may not appear in the data in some years.

[44] Note that when plotting the cohort-year-specific means in Figure 5, missing values of logarithmic real annual earnings (due to zero real annual earnings or missing values of real annual earnings) are ignored. Luckily, within the time period of interest for this thesis, the first ten years after graduation, only around 20,000 observations (or 1.4% of all observations) are ignored; roughly 14,000 of them are due to missing values and 6,000 due to zero earnings. Overall, when not limiting to the first ten years only around 1.53% of all observations ignored. Thus, composition bias is not a large issue.

Figure 6: Unemployment Profiles by Graduation Cohort



Notes: This figure presents the evolution of mean unemployment (fraction of unemployed) for graduation cohorts 1988–2004 at the national level. Dashed lines connect the unemployment experiences of all cohorts at one (black), two (green), five (red) and ten (orange) years of potential experience. An individual is defined as being unemployed if she is unemployed during the last week of the year. Computed using the FLEED sample. See Section 6.1 for more discussion.

following graduation similar to those who graduated just before the depression.

Figure 5 also shows that the mean real annual earnings of different cohorts seem to converge. Earnings two years after the year of graduation (green dashed line) still differ a lot between cohorts and the variation coincides with the business cycle variation. However, when looking at mean real annual earnings five years (red dashed line) and ten years (orange dashed line) after the year of graduation, there is clearly less variation across cohorts. This suggests that graduates who face adverse economic conditions at the beginning of their careers are on average able to catch up luckier cohorts. It is notable, however, that even after ten years the mean real annual earnings of cohorts who faced the deep depression early on in the labor market (cohorts 1988–1993) are still roughly 6–8% lower than of those who graduated after the depression (in 1996 and later). Finally, Figure 5 shows that for all graduation cohorts the evolution of earnings over time follows a similar pattern: mean logarithmic real annual earnings are a concave function of time. This implies that the growth rate of earnings is higher early on in the career and slows down over time. This result is consistent with findings from several other countries and supports the results of Topel and Ward (1992) who find that wage rates increase more rapidly early on in the career.

Figure 6 tells a similar story on the variation between cohorts and evolution over time

of the incidence of unemployment (fraction of unemployed).[45] Not surprisingly, the cohort-specific shares of unemployed are considerably lower than the unemployment rates for the overall population, showing that high-educated workers are less susceptible to unemployment, even when facing very adverse economic conditions. Nevertheless, the variation in the incidence of unemployment between graduation cohorts is considerable early on in the labor market. For example, in the year following the year of graduation (black dashed line), the share of unemployed among cohorts who graduated just before the depression (cohorts 1988 and 1989) was just 1.8%, whereas among the unlucky cohorts of 1992 and 1993 who graduated during the depression it was as high as 11–13%. We can also clearly see that during the depression, the shares of unemployed were much lower for older cohorts. For example, in 1993 the share of unemployed among the graduates who had just entered the labor market (cohort 1992) was 13.5%, whereas among those who graduated in 1988–1990, it was 5.6–7.4%. This suggests that, although the incidence of unemployment rises also among more experienced cohorts, more years of (potential) work experience helps to shield from adverse economic conditions.

As already noted for real annual earnings, Figure 6 also shows that there is convergence in the incidence of unemployment between graduation cohorts. Although there is still clear pro-cyclical variation in the shares of unemployed two years after the year of graduation (green dashed line), the variation is much smaller five years (red dashed line) and ten years (orange dashed line) after the year of graduation. This again suggests that the effect of economic conditions upon graduation dissipates over time. It should be noted, however, that this partly also reflects the fact that the majority of graduation cohorts faced much more favorable economic conditions five (and especially ten) years after the year of graduation. During more stable economic conditions (from the year 2000 and onwards), differences in the shares of unemployed between graduation cohorts are relatively small when graduates have two or more years of potential work experience.

To sum up the descriptive graphical analyses, economic conditions upon graduation on average clearly seem to have sizable and persistent effects on real annual earnings and the incidence of unemployment. However, as time passes the experiences of different graduation cohorts seem to converge, suggesting that the effects of initial economic conditions are not necessarily permanent but instead dissipate over time. Nevertheless, these descriptive analyses cannot necessarily be taken as evidence on the existence of a causal effect of initial economic conditions on later labor market outcomes. To be able to credibly have strong evidence on a causal mechanism from graphical inspections alone, we would have to compare the evolution of two otherwise similar graduation cohorts who faced considerably different economic conditions in the year of graduation but as similar conditions as possible in subsequent years. However, it is not necessarily clear that we can find cohorts of that sort in Figures 5 and 6. Thus, a more careful analysis in needed.

[45] There are no missing values with respect to the unemployment variable. However, as already noted, after the first ten years following graduation some individuals may not appear in the data in some years.

6.2 Effects on Earnings and Unemployment

I now move on to present the main results of this thesis concerning the effect of facing a high regional unemployment rate in the year of graduation on group-specific means of logarithmic real annual earnings, unemployment and receipt of unemployment benefits. As already discussed in Chapter 5, I use grouped panel data where the groups are defined by graduation cohort (year of graduation), denoted by c, and region of residence in the year of graduation, denoted by r.[46] For the results in this section, I look at the first ten years after the year of graduation for graduation cohorts 1988–2004 using the FLEED sample. There are 306 ($= 17 \times 18$) cr-groups in total and thus the grouped panel data contain 3060 group-year-observations.

Table 3 presents the results of estimating Equation 1 and shows how the regional unemployment rate in the year of graduation affects the average graduate. First, column (1) indicates that one year after graduation, real annual earnings decline by 2.1% in response to a percentage point higher regional unemployment rate. However, the effect on earnings seems to be remarkably persistent: the initial effect is reduced by roughly a quarter after five years and is halved only after 9–10 years. Moreover, the coefficients for each of the first ten years after the year of graduation are significant at the 1% significance level.[47] However, as Cockx and Ghirelli (2016, p. 168) discuss, interpreting the estimate of the effect in response to a percentage point increase in the unemployment rate is challenging because variations and levels seen in unemployment rates over time vary across countries depending on labor market institutions, for example. Furthermore, as already discussed in Section 3.1, the unemployment rates I compute using the FLEED data are not by themselves internationally comparable due to differences in the way unemployment is defined. Caveats like these in general make cross-country comparisons of the estimates obtained in the literature difficult. Thus, to illustrate the magnitude of the effects in a more transparent manner, we can compare a graduate who faces the mean regional unemployment rate in the year graduation to a graduate who faces a regional unemployment rate that is 6 percentage points higher (i.e. roughly a standard deviation higher, see Table 2). Initially, the adverse economic conditions upon graduation decrease the real annual earnings by 12.6%. Five years after graduation, real annual earnings are 8.9% lower, and ten years after graduation they are still 6%

[46] As noted in Section 5.1, the estimates obtained using this grouped data approach are identical to those obtained by estimating the same regression equation with the underlying individual-level microdata. This allows me to discuss the estimates *as if* they were obtained with individual-level data.

[47] As already noted, invalid observations with either zero or missing real annual earnings are not included when estimating the effects on earnings. To see whether regional economic conditions are associated with changes in the composition of earned income recipients, I estimated Equation 1 using the fraction of valid earnings observations in a cr-group in year t as the outcome variable. There is indeed a persistent and statistically significant negative effect on the fraction of valid observations, which indicates that the estimates in Column (1) of Table 3 are subject to selection bias. However, all the estimates in all years of potential work experience are very small, indicating roughly a 0.12 percentage points reduction in the fraction of valid observations in response to percentage point increase in regional unemployment rate. Therefore the bias does not affect the validity of the estimates in any meaningful way. The same conclusion holds for all the sensitivity and heterogeneity analyses that follow.

lower. However, I emphasize that the earnings variable here refers to *earned income* and therefore does not take into account e.g. received unemployment benefits. Thus the effect on for example total real disposable income is likely to be somewhat smaller in magnitude and persistence.

Column (2) presents the effect of facing a percentage point higher regional unemployment rate in the year of graduation on unemployment (being unemployed during the last week of the year) for the average graduate. In the year following the year of graduation, the coefficient is 0.0033, meaning that a percentage point higher unemployment rate upon graduation increases the probability of unemployment by 0.33 percentage points. Since roughly 5.5% (3.6%) of graduates are unemployed in the FLEED sample in the first year after graduation (each year during the whole time period) (see Tables A7 and 2), this implies roughly a 6% (9.2%) increase in the probability of unemployment. The effect is roughly halved after six years and is no longer significant at the 5% significance level after seven years. Thus the effect on unemployment is not as persistent as the effect on real annual earnings. Continuing the earlier hypothetical comparison, a graduate who faces a 6 percentage points higher regional unemployment rate upon graduation has a 2 percentage points higher probability of being unemployed in the following year. After six years, she still has roughly a percentage point higher probability of being unemployed.

Finally, Column (3) presents the effects on the probability of receiving unemployment benefits for the average graduate. I have chosen to also look at this outcome variable because the reference period used in defining unemployment (last week of the year) is rather arbitrary and likely to understate the incidence of unemployment during a given year; for example, it does not consider e.g. individuals who are unemployed earlier on in the year but no longer unemployed by the end of the year. Furthermore, as noted in Section 4.3, accepting a part-time job or a very short-term full-time job can leave an individual entitled to partial unemployment benefits. For these reasons we might expect the effects on the receipt of unemployment benefits to be higher. As can be seen in Column (3), this is indeed the case. A percentage point higher regional unemployment rate in the year of graduation is associated with roughly a 0.9 percentage points increase in the probability of receiving unemployment benefits in the year following graduation. Comparing this to the estimates in Column (2), the initial effect is almost three times larger than the effect on unemployment. The initial effect is almost cut in half (0.49 percentage points) already in the following year. Three to six years after the year of graduation, the effect is slightly lower (0.29–0.39 percentage points), but still statistically highly significant (at the 1% significance level). Seven years after graduation, the effect falls to just 0.19 percentage points, though being still significant at the 5% level, and is no longer significant afterwards. Given that receiving unemployment benefits is obviously highly correlated with being unemployed, it is not surprising that the persistence of the effect is similar to the effect on unemployment.

Table 3: Effects On Group-Specific Means of Logarithmic Real Annual Earnings, Unemployment and Receipt of Unemployment Benefits: Graduation Cohorts 1988–2004.

Effect by Years of Potential Experience (β_e)	(1) Log Earnings	(2) Unemployment	(3) Unemp. Benefits
1	-0.0210***	0.0033***	0.0089***
	(0.0023)	(0.0005)	(0.0012)
2	-0.0178***	0.0024***	0.0049***
	(0.0023)	(0.0005)	(0.0011)
3	-0.0163***	0.0025***	0.0036***
	(0.0023)	(0.0004)	(0.0010)
4	-0.0151***	0.0024***	0.0039***
	(0.0022)	(0.0004)	(0.0009)
5	-0.0148***	0.0021***	0.0034***
	(0.0023)	(0.0004)	(0.0010)
6	-0.0140***	0.0018***	0.0029***
	(0.0023)	(0.0004)	(0.0009)
7	-0.0125***	0.0013***	0.0019**
	(0.0023)	(0.0004)	(0.0009)
8	-0.0115***	0.0008*	0.0007
	(0.0022)	(0.0004)	(0.0009)
9	-0.0105***	0.0005	-0.0003
	(0.0022)	(0.0004)	(0.0009)
10	-0.0099***	0.0005	-0.0009
	(0.0022)	(0.0004)	(0.0009)
R^2	0.964	0.791	0.924
Observations	3060	3060	3060

Notes: This table contains the results from estimating Equation 1 for group-specific means of logarithmic real annual earnings, unemployment and receipt of unemployment benefits using the whole FLEED sample. Groups are defined by graduation cohort (year of graduation) and region of residence in the year of graduation. Standard errors clustered by graduation cohort and region of residence in the year of graduation are in parentheses.
Statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

We can continue the hypothetical comparison between two average graduates, one belonging to a "lucky" cohort and the other to an "unlucky" cohort facing six percentage points higher regional unemployment rate in the year of graduation. One year after graduation, the unlucky graduate has roughly a 5.4 percentage points higher probability of receiving unemployment benefits. Given that on average roughly 21.9% (11.8%) of the individuals receive unemployment benefits in the first after graduation (each year during the whole time period) in the FLEED sample (see Tables A7 and 2), this corresponds to a roughly 24.7% (45.5%) higher probability. Two to six years after graduation, the probability of receiving unemployment benefits is still 1.7–2.9 percentage points higher. After that, the difference is at most 1.1 percentage points and becomes statistically insignificant after seven years.

6.3 Heterogeneity

In this section I provide a range of heterogeneity analyses. After that I move on to assess the sensitivity and robustness of the results in the next section. To start, I estimate Equation 1, using (group-specific means of) logarithmic real annual earnings and unemployment as outcome variables, separately for cohorts that faced the deep 1990s depression upon or shortly after graduation (cohorts 1988–1995) and cohorts who graduated after the depression and faced generally more favorable economic conditions (cohorts 1996–2004). Considering that the 1990s depression was such an unusually deep economic contraction even from an international perspective, this analysis is reasonable and serves as a check for whether cohorts who faced the depression drive the results presented in the previous section. If this is indeed the case and the results change significantly, the external validity of my main results to e.g. more recent Finnish graduation cohorts might be questionable.

Columns (1)–(2) and (3)–(4) in Table 4 present the estimates for cohorts 1988–1995 and 1996–2004, respectively. As is evident, the results differ from those obtained for the whole sample. An analysis restricting only to cohorts 1988–1995 indicates that the regional unemployment rate in the year of graduation only has a statistically significant effect on real earnings in the first two years after graduation (but the effect at two years of potential experience is not significant at the 5% level). Furthermore, the effects are considerably smaller in magnitude compared to those obtained for the whole sample: for example, the initial effect is only half of that estimated for all graduation cohorts (cf. Column (1) of Table 3). In contrast, the effects appear to be more similar and persistent for unemployment: the initial effect and further effects up to the fifth year after graduation are roughly 20% smaller in magnitude in comparison to the estimates for all cohorts. Since the effects are no longer statistically significant at the 5% level after five years, the effects for unemployment are less persistent than for the whole sample. These results imply that, among the cohorts who all faced the 1990s depression, those who

faced relatively even more adverse initial economic conditions upon graduation seem to face larger costs. However, the negative effects for real annual earnings are much smaller and more short-lived than among all cohorts but more similar for unemployment. In conclusion, the smaller effects of initial economic conditions among graduation cohorts who experienced the deep 1990s depression indicate that the depression hurt their careers in more even ways: the timing of graduation seems to matter less than among all cohorts.

When restricting the analysis to cohorts 1996–2004, who generally faced more favorable economic conditions upon graduation and afterwards, the effects on both real annual earnings and unemployment differ considerably from those obtained for the whole sample. First, as can be seen in Column (3), the negative effects on real annual earnings are clearly less persistent: the effects are statistically significant at the 5% significance level only for the first five years after graduation. A percentage point increase in the regional unemployment rate in the year of graduation leads to a decline of roughly 2.4% in real annual earnings in the first year following graduation. This initial effect is similar to that obtained for the whole sample. However, the effect is halved already by the third year after graduation. Thus the effects of earnings are on average less persistent among these cohorts. Furthermore, there seem to be no effects on unemployment: a χ^2-test of the null hypothesis that the effect on unemployment is zero in each of the first ten years after graduation is not rejected (see Column (4)).[48] Thus, for these cohorts, facing adverse economic conditions upon graduation does not seem to affect the probability of being unemployed, at least on average.

In sum, we can conclude that the effects estimated for the whole sample are driven to a sizable extent by the cohorts who faced the 1990s depression. This finding accentuates the fact that the depression was indeed an unusually disruptive experience that hurt the careers of university graduates for a long time. Given that it was already seen in Figure 5 that the mean real annual earnings of these especially unlucky cohorts continue to lag behind the mean real annual earnings of other cohorts even ten years after graduation, these results are not particularly surprising. Furthermore, the differences between cohorts who did and did not face the depression highlights a difference between the mechanisms behind the negative effects on real earnings. For the unlucky cohorts who faced the depression, a persistent effect on unemployment is a relevant channel through which the adjustment to a depressed labor market happens. In contrast, unemployment is not an important mechanism for those who graduated after the depression, indicating that other mechanisms seem to be more relevant; these alternative channels could include part-time employment, wage and working hours reductions, poorer quality of

[48] I use a χ^2-test instead of the conventional F-test when performing hypothesis tests of linear restrictions because the data I use is *not* a random sample from the population of interest, but almost the whole population itself. Because of the large sample size, we can use asymptotic distributional theory and assume that the regression model error terms are likely to be close to normally distributed by the central limit theorem. Thus in this case, I carry the joint linear hypothesis tests using JF as the test statistic, where F is the conventional F-statistic and J the number of linear restrictions being tested, and use the critical values of the χ^2_J-distribution. See e.g. Greene (2008, Section 5.4).

Table 4: Effects on Group-Specific Means of Logarithmic Real Annual Earnings and Unemployment: Graduation Cohorts 1988–1995 vs. 1996–2004.

	Cohorts 1988–1995		Cohorts 1996–2004	
Effect by Years of Pot. Exp (β_e)	(1) Log Earnings	(2) Unemployment	(3) Log Earnings	(4) Unemployment
1	-0.0103**	0.0028***	-0.0237***	-0.0022*
	(0.0044)	(0.0009)	(0.0063)	(0.0012)
2	-0.0073*	0.0018*	-0.0152***	-0.0009
	(0.0043)	(0.0009)	(0.0048)	(0.0007)
3	-0.0065	0.0020**	-0.0123***	-0.0006
	(0.0042)	(0.0009)	(0.0043)	(0.0006)
4	-0.0059	0.0021**	-0.0106**	-0.0009
	(0.0042)	(0.0009)	(0.0041)	(0.0006)
5	-0.0064	0.0019**	-0.0087**	-0.0009
	(0.0042)	(0.0009)	(0.0042)	(0.0007)
6	-0.0061	0.0015*	-0.0074*	-0.0004
	(0.0043)	(0.0009)	(0.0039)	(0.0007)
7	-0.0049	0.0009	-0.0058	-0.0002
	(0.0042)	(0.0009)	(0.0038)	(0.0007)
8	-0.0042	0.0004	-0.0044	-0.0004
	(0.0042)	(0.0009)	(0.0036)	(0.0006)
9	-0.0034	0.0000	-0.0043	-0.0004
	(0.0042)	(0.0009)	(0.0035)	(0.0006)
10	-0.0027	-0.0001	-0.0049	-0.0003
	(0.0042)	(0.0009)	(0.0033)	(0.0006)
χ^2_{10}-*value*	90.360	111.197	27.452	13.809
p-value	0.000	0.000	0.002	0.182
R^2	0.965	0.850	0.974	0.649
Observations	1440	1440	1620	1620

Notes: This table contains the results from estimating Equation 1 for group-specific means of logarithmic real annual earnings and unemployment separately for graduation cohorts 1988–1995 and 1996–2004 using the FLEED sample. Groups are defined by graduation cohort (year of graduation) and region of residence in the year of graduation. Standard errors clustered by graduation cohort and region of residence in the year of graduation are in parentheses.
The χ^2_{10} and p values correspond to the χ^2-test of joint significance $H_0 : \beta_1 = \cdots = \beta_{10} = 0$.
Statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

initial employment, skill mismatch, task down-grading etc.

Next, I look at the effects on real annual earnings and unemployment separately for the average male and female graduate. Potential gender differences may be driven by a combination of differences in the choices of field of study, occupation and industry, among others.[49] Table 5 presents the results of estimating Equation 1 separately for males (column (1) and (2)) and females (columns (3) and (4)) using all graduation cohorts. As can be seen, the effects on both real annual earnings and unemployment are similar in terms of persistence: the effects on earnings are statistically highly significant for each of the first ten years after graduation, while the effects on unemployment are statistically significant at least at the 5% level for the first seven years. However, there are gender differences in terms of magnitude: the effects on real annual earnings for the average female graduate are smaller in each of the first ten years since graduation. In contrast, there appear to be no clear gender differences in the effects on unemployment, neither in terms of magnitude nor persistence.

Much more salient gender differences emerge when I restrict the analysis to cohorts 1996–2004. These results are presented in Table 6. The effects on real annual earnings are much larger for the average male graduate, both in terms of magnitude and persistence. In response to a percentage point increase in the regional unemployment rate in the year of graduation, real annual earnings fall by 3.1% in the year following graduation. The effect is halved only after 6–7 years and remains statistically highly significant in each of the first ten years after graduation. Remarkably, these effects are closer to those found for the whole sample and all graduates in Table 3. In contrast, the effects for the average female graduate are much smaller and more short-lived. The effect in the year following graduation is roughly –1.8% (i.e. 40% smaller than for the average male graduate), but is cut to half already in the following year. Furthermore, the effects on earnings are not statistically significant at the 5% level from the third year and beyond. When turning to the effects on unemployment, there seem to be no effects for either gender. For men, the χ^2-test does not reject the null hypothesis that the coefficients for the first ten years are all jointly zero, indicating no effects on unemployment. For women, the effect of unemployment in statistically insignificant in all years, but the χ^2-test rejects the null hypothesis that the effects on unemployment are jointly zero in each of the first ten years at the 5% level. Thus while there could be a small and very short-lived effect on unemployment for the average female, it is probably not economically significant.

The clear gender differences in the effects on earnings probably reflect the fact that women are more likely to hold a degree in fields relating to e.g. education and health care, for which the primary employing sector is the public sector. Men on the other hand, are more likely to work in the private sector, where demand for the final prod-

[49] Furthermore, it might also be that women can e.g. strategically exit the labor force due to maternity leave if they face adverse economic conditions upon or shortly after graduation.

Table 5: Effects On Group-Specific Means of Logarithmic Real Annual Earnings and Unemployment Separately for Males and Females: Cohorts 1988–2004.

	Males		Females	
Effect by Years of Pot. Exp. (β_e)	(1) Log Earnings	(2) Unemployment	(3) Log Earnings	(4) Unemployment
1	-0.0213***	0.0032***	-0.0191***	0.0033***
	(0.0030)	(0.0006)	(0.0028)	(0.0006)
2	-0.0183***	0.0026***	-0.0157***	0.0023***
	(0.0030)	(0.0006)	(0.0027)	(0.0006)
3	-0.0172***	0.0025***	-0.0141***	0.0025***
	(0.0029)	(0.0006)	(0.0026)	(0.0006)
4	-0.0164***	0.0026***	-0.0124***	0.0023***
	(0.0029)	(0.0006)	(0.0025)	(0.0006)
5	-0.0160***	0.0023***	-0.0123***	0.0019***
	(0.0029)	(0.0006)	(0.0026)	(0.0006)
6	-0.0147***	0.0018***	-0.0119***	0.0017***
	(0.0030)	(0.0005)	(0.0026)	(0.0006)
7	-0.0133***	0.0012**	-0.0104***	0.0013**
	(0.0029)	(0.0006)	(0.0026)	(0.0006)
8	-0.0119***	0.0006	-0.0097***	0.0010*
	(0.0029)	(0.0005)	(0.0026)	(0.0006)
9	-0.0106***	0.0005	-0.0090***	0.0005
	(0.0028)	(0.0005)	(0.0025)	(0.0006)
10	-0.0101***	0.0002	-0.0083***	0.0007
	(0.0028)	(0.0005)	(0.0025)	(0.0006)
R^2	0.961	0.683	0.917	0.691
Observations	3060	3060	3060	3060

Notes: This table contains the results from estimating Equation 1 for group-specific means of logarithmic real annual earnings and unemployment separately by gender using the whole FLEED sample. Groups are defined by graduation cohort (year of graduation) and region of residence in the year of graduation. Standard errors clustered by graduation cohort and region of residence in the year of graduation are in parentheses.
Statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Effects On Group-Specific Means of Logarithmic Real Annual Earnings and Unemployment Separately for Males and Females: Cohorts 1996–2004.

	Males		Females	
Effect by Years of Pot. Exp. (β_e)	(1) Log Earnings	(2) Unemployment	(3) Log Earnings	(4) Unemployment
1	-0.0306***	-0.0014	-0.0180***	-0.0026
	(0.0100)	(0.0016)	(0.0065)	(0.0018)
2	-0.0237***	-0.0005	-0.0088**	-0.0010
	(0.0072)	(0.0013)	(0.0045)	(0.0012)
3	-0.0204***	-0.0004	-0.0064*	-0.0006
	(0.0063)	(0.0012)	(0.0038)	(0.0009)
4	-0.0201***	-0.0006	-0.0038	-0.0011
	(0.0057)	(0.0012)	(0.0037)	(0.0009)
5	-0.0171***	-0.0010	-0.0027	-0.0008
	(0.0057)	(0.0012)	(0.0041)	(0.0010)
6	-0.0158***	-0.0013	-0.0011	0.0003
	(0.0055)	(0.0012)	(0.0040)	(0.0010)
7	-0.0140***	-0.0013	0.0006	0.0007
	(0.0054)	(0.0012)	(0.0041)	(0.0010)
8	-0.0113**	-0.0016	0.0012	0.0005
	(0.0052)	(0.0012)	(0.0042)	(0.0009)
9	-0.0109**	-0.0016	0.0013	0.0004
	(0.0052)	(0.0012)	(0.0040)	(0.0010)
10	-0.0124**	-0.0014	0.0016	0.0006
	(0.0049)	(0.0012)	(0.0039)	(0.0010)
χ^2_{10}-*value*	24.292	10.525	18.141	19.273
p-value	0.007	0.396	0.053	0.037
R^2	0.965	0.457	0.939	0.557
Observations	1620	1620	1620	1620

Notes: This table contains the results from estimating Equation 1 for group-specific means of logarithmic real annual earnings and unemployment separately by gender using graduation cohorts 1996–2004 of the FLEED sample. Groups are defined by graduation cohort (year of graduation) and region of residence in the year of graduation. Standard errors clustered by graduation cohort and region of residence in the year of graduation are in parentheses.
The χ^2_{10} and p values correspond to the χ^2-test of joint significance $H_0 : \beta_1 = \cdots = \beta_{10} = 0$.
Statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

uct and labor demand are usually more responsive to business cycle variation. As e.g. Liu et al. (2016) find in their study of Norwegian graduates, their results on earnings and unemployment are driven largely by graduates working in the private sector. My findings seem to be consistent with this previous evidence. Finally, the small effects on unemployment for both genders among the post-depression cohorts 1996–2004 complements the finding already seen in Table 4 that the 1990s depression is driving the negative effects on unemployment found for the whole sample.

6.4 Sensitivity and Robustness

Alternative Definitions of Unemployment

Finally, I turn to provide a number of sensitivity and robustness inspections. To start, I repeat the main analyses concerning the effects on unemployment using two alternative definitions of unemployment (see Section 3.1): dummy variables taking value one if the individual is unemployed for at least (i) one or (ii) three month(s) during the year. Using the *cr*-group-specific means of these outcome variables (i.e. the fractions of unemployed in the group) as outcome variables, I estimate Equation 1 both for the whole sample and for cohorts 1996–2004. The results are presented in Table A1. As can be seen in columns (1) and (2), for the whole sample the persistence of the unemployment effects are similar to those reported in Table 3. However, the point estimates are considerably larger when using these alternative unemployment variables: the initial effect in the first year after graduation is roughly three times (twice) as large as the initial effect reported in Table 3 if one (three) month(s) is used as the threshold in the definition of unemployment. After that, for both variable definitions the effects are for the most part twice as large as those reported in Table 3. Thus we can conclude that there are indeed sizable effects on unemployment, although a significant share of graduates who experience unemployment at some point during the year are able to find employment by the end of the year (since the effects in Table 3 are smaller).

Columns (3) and (4) of Table A1 indicate that for cohorts who graduated after the 1990s depression (under generally more normal business cycle conditions), the effects on unemployment using the alternative definitions are quite similar to those obtained using the main definition (cf. Column (4) of Table 4). The statistically significant negative unemployment "effects" at years 8–10 in Column (3) may seem surprising at first, but they most likely arise due to the fact that most graduation cohorts (cohorts 1998–2004) experienced the recent economic recession, which started in 2008, within the first ten years since graduation. Because those who did not experience it within the same time period (cohorts 1996–1997) instead faced more adverse economic conditions upon graduation (see Figures 2 and 6), this produces a positive correlation for outcomes at years 8–10. Since the effects for these years in Column (4) are not significant at the

5% level, this correlation is only short-lived. All in all, the results I obtain concerning the effects on unemployment are insensitive to alternative definitions of unemployment.

Effects on Obtaining a Graduate Degree

To continue, I check next whether facing adverse economic conditions upon graduation affects the choice of obtaining a graduate degree. This is possible because facing adverse economic conditions upon graduation is likely to affect expected earnings negatively and thus reduce the opportunity cost of further schooling. If economic conditions upon graduation do affect the graduate school decision, further accumulation of human capital through obtaining a graduate degree can be seen as a mechanism through which adverse economic conditions affect university graduates. Roughly 12,800 graduates, or 9% of all graduates in the FLEED sample, had obtained a graduate degree within the first ten years since obtaining a Master's degree.

To see whether regional economic conditions (proxied by regional unemployment rate) in the year of graduation affect the probability of obtaining a graduate degree, I estimate the following linear probability model[50] using the whole FLEED sample with individual-level observations:

$$D_{icr} = \alpha_1 + \beta UR_{cr} + \theta_r + \chi_c + v_{icr}, \quad (2)$$

where D_{icr} is a dummy variable taking value one if individual i belonging to graduation cohort c and living in region r in the year of graduation has obtained a graduate degree by the tenth year since graduation, α_1 is the constant term, U_{cr} is the regional unemployment rate in the year of graduation, χ_c and θ_r denote fixed effects with respect to graduation cohort and region of residence in the year of graduation, respectively, and v_{icr} is the error term. Standard errors are clustered at the level of graduation cohort and region of residence in the year of graduation. As can be seen in Column (1) of Table A2, regional unemployment rate upon graduation has no effect on the probability of obtaining a graduate degree. This indicates that regional economic shocks do not affect the graduate school decision. I repeat the analysis also using major regional and national unemployment rates as main regressors.[51] These specifications indicate a positive, albeit very small, effect on obtaining a graduate degree (see Columns (2) and (3) of Table A2). Furthermore, the small R^2 values of all three models indicate that none of them has a decent capability of explaining the choice of obtaining a graduate

[50] I chose to report the results from a linear probability model because of the easier interpretation of the estimates (as changes in the probability of obtaining a graduate degree). Using probit or logit specifications, which are usually more appropriate in the case of a binary dependent variable, offer qualitatively similar results and are thus not reported.

[51] When using major regional unemployment rates, I in essence use the model of Equation 2 but instead control for fixed effects of the major region of residence in the year of graduation and cluster the standard errors at the level of graduation cohort and major region of residence in the year of graduation. When using national unemployment rates, I only include graduation cohort fixed effects and cluster standard errors at the graduation cohort level.

degree. In conclusion, economic conditions upon graduation at most seem to only have a small effect on the choice of obtaining a graduate degree, but this appears not to be a significant way of coping with negative economic shocks.[52]

Effects on Regional Mobility

Next, I study whether economic conditions upon graduation affect regional mobility. As discussed in Section 5.2, previous evidence suggests that highly educated young workers, such as university graduates, do respond to changes in local labor market conditions. If the effect on regional mobility is sizable, it could be argued that the regional unemployment rate in the year of graduation is not a particularly relevant source affecting labor market outcomes after graduation. Overall, the graduates in the FLEED sample change regions relatively often: roughly 11% (17%) of them move to another region in the year (first two years) after graduation, and roughly a third of them move at least once during the first ten years since graduation. To investigate the effects on regional mobility, I estimate the main regression model of Equation 1 using the *cr*-group-specific fraction of those who moved to another region in calendar year t as the outcome variable. The results are shown in Table A3. For the whole sample (see Column (1)), none of the effects on regional mobility in the first ten years after graduation are individually statistically significant at the 5% significance level. However, the χ^2-test on the joint significance of entire vector of effects in the first ten years indicates that there are some (apparently small) effects on mobility.

Columns (2) and (3) of Table A3 contain the results of the same analysis repeated separately for the cohorts who faced the 1990s depression and for the cohorts who graduated after the depression, respectively. Similarly to the results for the whole sample, there seem to be relatively small positive effects on regional mobility in the first couple of years after graduation among cohorts 1988–1995, even though only one of the individual coefficients is statistically significant at the 5% level. In contrast, when restricting the analysis only to cohorts 1996–2004, who graduated after the depression, there are much larger and statistically significant positive effects on regional mobility in the first couple of years following graduation.

However, it is difficult to interpret these differences between cohorts since we cannot take these results at face value: we cannot necessarily conclude that regional mobility after graduation in response to adverse economic conditions was less common among those who faced the depression than among those who graduated afterwards. This is because, as already discussed in Section 5.2, students became able to register the region where

[52]It is worth noting that some graduates may initially opt to pursue graduate studies when facing adverse economic conditions, but later choose to drop out of the program (without completing the degree) and enter the labor market once labor market conditions improve. This can be the case if graduates see that this choice would send a better signal to future employers, for example. Unfortunately, studying to what extent this actually happens is something I cannot do with the FLEED data alone.

they study as their region of residence in 1994 by the enactment of the Municipality of Residence Act. It is clear that the law creates non-random measurement error to the outcome variable since university students can *choose* whether to register the region (or more precisely, the municipality) where they study as their region (municipality) of residence.[53] This means that the estimates in Table A3 are biased, and the magnitude and direction of the bias is very hard to assess since I do not have data on the region of the university. Thus, the magnitude and persistence of the effects on regional mobility I obtain should be treated with caution. Nevertheless, I view it as unlikely that the bias would be large enough to change the overall result that graduates on average do respond to adverse regional economic shocks by increasing regional mobility. Thus, I conclude that my results concerning regional mobility are consistent with previous evidence of Oreopoulos et al. (2012, Section III) for Canada and Wozniak (2010) for the U.S. In contrast to my findings, Liu et al. (2016, Section 5.1) find no effects on regional mobility (in the year following graduation) for Norway.[54]

Major Regional Specification

As discussed in Section 3.1, for the main results I chose to use regions as the relevant geographical units. However, it might be that the regional labor market does not in fact constitute the relevant labor market for university graduates. Thus, I next study whether using a specification at the major regional level changes the results in a significant way. In essence, I use the same regression model as in Equation 1, but instead I use groups defined by graduation cohort and major region of residence in the year of graduation, the major regional unemployment rate in the year of graduation as the main regressor, and control for fixed effects with respect to major region of residence in the year of graduation. Since the descriptive statistics in Table 2 show that the mean (and median) and standard deviation of the major regional unemployment rate in the year of graduation are very similar in magnitude to those of the regional unemployment rates, we can directly compare the point estimates obtained from the regional and major regional specifications. Large discrepancies between the results obtained using the two specifications would suggest that adverse economic shocks at the major regional level upon graduation are associated with unobservable changes in the characteristics of regional graduation cohorts.[55]

[53] For example, before the enactment of the law a graduate, who studied in a region which differed from her region of residence, would be classified as a mover if she decides after graduation to stay in the region where she studied. However, after the law came to effect, a similar graduate could be classified as a mover or a non-mover depending on whether she decided while studying to register the region where she studied as her region of residence.

[54] Liu et al. (2016, Section 5.1) use both the region of the university and the region of residence in the year of graduation when estimating the effects on regional mobility. In both cases, their conclusion is the same.

[55] This issue was highlighted by Oreopoulos et al. (2012, p. 13), although in their study in the context of potential differences between provincial and national level specifications with Canadian data.

Unfortunately, a problem with the major regional specification is that number of possible groups to be used when aggregating the individual-level microdata is much smaller than with the regional specification: there are at most 68 ($= 4 \times 17$) different "major region – graduation cohort" groups (since graduates from Åland are excluded). The low number of possible groups means that when clustering the standard errors at the relevant group level, I may have too few clusters and thus overreject null hypotheses (see Angrist and Pischke 2009, Chapter 8, and Cameron and Miller 2015, Section VI). This "few clusters" problem is further exacerbated by the fact that I use weighted least squares estimation that places different weights to different clusters (see the discussion in Cameron and Miller 2015, Section VI).

The above caveats notwithstanding, I present the results concerning the effects on real annual earnings and unemployment from the major regional specification in Table A4. Columns (1) and (2) show that the results for the whole sample are relatively similar to the main results from the regional specification (cf. Table 3), especially the effects on unemployment. The effects on earnings are somewhat larger, though. When limiting the analysis to the post-depression cohorts 1996–2004 (columns (3) and (4)), the results again appear to be broadly similar to those obtained with the regional specification (cf. Table 4), even though the point estimates for earnings are smaller after the first year and indicate less persistent effects on earnings. The major regional specification also appears to show that there are negative effects for unemployment, which contrasts with be regional specification. However, given that the number of clusters used for computing standard errors is rather low (only 36), the estimated standard errors are likely to be biased downwards and thus the effects are most likely not statistically significant in reality. All in all, the similarity of the point estimates from the two specifications allow me to conclude that my main results are quite insensitive to using an alternative specification with panel data grouped at the major regional level.

Selective Timing and Place of Graduation

A possible threat to the validity of the empirical analyses is selective timing of graduation, as I already discussed in Section 5.2. Since I do not have information on the duration of education, addressing this issue is admittedly challenging. However, if there is clear evidence of strategic delay of graduation, we would expect that the regional number of graduates should fall during years with adverse regional economic conditions (a high regional unemployment rate). To investigate this, I estimate the following model:

$$ln(N_{cr}) \;=\; \alpha_2 \;+\; \beta_1 U_{cr} \;+\; \theta_r \;+\; \chi_c \;+ w_{cr}, \tag{3}$$

where N_{cr} is the number of graduates who belong to graduation cohort c and whose region of residence in the year of graduation is r, α_2 is the constant term, U_{cr} is the regional unemployment rate in the year of graduation facing those belonging to grad-

uation cohort c and living in region r, θ_r and χ_c denote fixed effects with respect to the region of residence in the year of graduation and graduation cohort, respectively, and w_{cr} is the error term. Standard errors are clustered at the regional level. Strong evidence of strategic delay of graduation would imply a negative correlation between regional unemployment rate and the logarithm of the size of the regional graduation cohort, after controlling for regional and graduation cohort fixed effects. In other words, we should expect the coefficient of regional unemployment rate in Equation 3, β_1, to be negative. The results are presented in Column (1) of Table A5. As can be seen, there does not seem to be a negative correlation between regional unemployment rate and (the logarithm of) the size of the regional graduation cohort. If a linear or quadratic graduation cohort trend is used instead of graduation cohort fixed effects, the conclusion does not change (see Columns (2) and (3) of Table A5); if anything, there seems to be a small positive correlation. However, given that the regional graduation cohort sizes are generally quite small (as can be seen in Table A8), the magnitudes of estimates (0.04–0.1) are not substantial.[56] This analysis suggests that selective timing of graduation is not a large concern.

Another threat to the validity of the main results mentioned in Section 5.2 is selective place of graduation. As already discussed, this is due to the fact that in the FLEED data, the information on the region of residence refers to the situation during the last day of the year. This allows strategic migration to other regions in response to adverse economic shocks for individuals who graduated earlier during the year. As an attempt to inspect whether selective place of graduation is an issue, I investigated whether there is a positive correlation between the probability that the region of residence in the year of graduation and in the year *before* the year of graduation are different and the regional unemployment rate in the year of graduation in the region of residence of the year before the year of graduation.[57] More specifically, I estimated a modified version of Equation 2 where the outcome variable is a dummy variable taking value one if the individual's region of residence in the year of graduation differs from the region of residence in the previous year, and where the region used for defining the main regressor, regional fixed effects and clustering standard errors is the region of residence in the year before the year of graduation. Fortunately, the results indicate that there is no statistically significant correlation. This means that selective region of graduation seems to not be

[56] I did the same analyses also using cohort sizes at the major regional and national level (using major regional/national unemployment rates as regressors). In both cases, the conclusion remained the same and the results are thus not reported. In the major regional specification, I controlled for fixed effects with respect to major region of residence in the year of graduation instead and clustered standard errors at the major regional level. In the national specification, no other regressors besides national unemployment rate in the year of graduation were used, but in estimations I used both White heteroskedasticity-consistent and Newey-West autocorrelation-heteroskedasticity-consistent standard errors (allowing autocorrelation up to 1–2 lags).

[57] Note that in order to use the information on the region of residence in the year before the year of graduation, all individuals belonging to graduation cohort 1988 have to be omitted from the analysis since the FLEED data start from the year 1988. I also dropped the individuals for which the region of residence in the year before graduation was Åland. Nevertheless, 133,710 individuals (roughly 94% of all individuals) from the FLEED sample remain for the analyses. The region of residence in the year of graduation differs from that of the previous year for roughly 18.3% of these individuals.

a large concern, and therefore I have chosen not report the results.[58]

Isolating the Effect of Initial Regional Unemployment Rate

As discussed by Oreopoulos et al. (2012), a graduate who faces adverse regional economic conditions upon graduation is more likely to face adverse economic conditions also after the year of graduation. This means that the main regressor used in the analyses, the regional unemployment rate in the year of graduation, can be correlated with regional unemployment rates in the years following the year of graduation. Therefore, the results that have been presented thus far summarize the *cumulative* effects of the initial regional unemployment rate upon graduation *and* the effects of subsequent regional unemployment rates. Oreopoulos et al. (2012) note that, if the effects of these subsequent unemployment rate shocks are not controlled for, the estimator of the effect of the regional unemployment rate in the year of graduation in potential experience year e in Equation 1, β_e, has the following omitted variable bias formula:

$$\text{plim } \hat{\beta}_e = \beta_e + \sum_{d=1}^{e} \frac{\text{cov}(U_{cr0}, U_{cr_d d})}{\text{var}(U_{cr0})} \beta_{e,d}$$

where $\beta_{e,d}$ is the effect on the outcome variable in experience year e of the regional unemployment rate in experience year d (where $d < e$) and $U_{cr_d d}$ is the regional unemployment rate an individual belonging to graduation cohort c faces in experience d in the region of residence in that year, r_d. In other words, the estimator β_e captures the effect of the initial unemployment rate in the year of graduation *and* the weighted sum of the unemployment rates in further years, where the weights are determined by how large the term $\text{cov}(U_{cr0}, U_{cr_d d})$ is, i.e. how strongly the regional unemployment rate in experience year d is correlated with the regional unemployment rate in the year of graduation. The autocovariance structures of further regional unemployment rates with the regional unemployment rate in the year of graduation are summarized in Panels (a) and (b) of Figure A2 (see the figure notes for information on how the autocovariances are estimated). Among the whole sample (see Panel (a)), the subsequent regional unemployment rates are clearly positively correlated with the initial unemployment rate in first three years and show small negative correlation from the fifth year on. The structure is initially similar also among cohorts 1996–2004 (see Panel (b)). However, the correlation turns slightly positive in later years. Again, this is likely due to the fact that most cohorts among graduation cohorts 1996–2004 faced the recent recession which started in 2008.

While estimating the cumulative effects is important and indicates the total costs of graduating upon adverse economic conditions, it is also interesting to know to what extent the economic conditions in the year of graduation alone are responsible for the

[58] Using major regions instead of regions or probit/logit specifications do not change the conclusion.

total cumulative effects. To isolate the effect of the regional unemployment rate in the year of graduation, net of any correlated shocks in the years following graduation, I follow a method similar to that used by Oreopoulos et al. (2012) and Liu et al. (2016). First, I aggregate the individual-level microdata and use grouped panel data where groups are defined by graduation cohort (c), region of residence in the year of graduation (r) and current region of residence (r_e), and where observations are annual group-specific means of outcome variables ($\bar{y}_{crr_et}$). After that I estimate the following model:

$$\begin{aligned}\bar{y}_{crr_et} \;=\; \alpha \;+\; \sum_{e=1}^{10}\beta_e\, UR_{cr0} \;+\; \sum_{e=1}^{10}\beta_{e,1}\, UR_{cr_11} + \;\ldots\; + \sum_{e=1}^{10}\beta_{e,9}\, UR_{cr_99} \qquad (4)\\ +\; \phi_t \;+\; \theta_r \;+\theta_{r_e} \;+\; \gamma_e \;+\; \chi_c \;+\; \theta_r \times \gamma_e \;+\; u_{crr_et},\end{aligned}$$

where θ_{r_e} denotes fixed effects with respect to the current region of residence, $\beta_{e,d}$ is the effect of the unemployment rate in the region of residence in potential experience year d (where $d \in \{1, ..., 9\}$), UR_{cr_dd}, on the outcome variable in experience year e, u_{crr_et} is the error term, and the other terms are as in Equation 1. I impose the restriction $\beta_{e,d} = 0$ when $e \in \{1, ..., d\}$, in other words that the regional unemployment rate in experience year d can only affect the outcomes after that year. In this specification, the coefficientof interest, β_e, captures the effect in experience year e of the initial regional unemployment rate in the year of graduation, controlling for the effects of the further regional unemployment rates a graduate faces. The results are reported in Table A6. For the whole sample (columns (1) and (2)), the effects on unemployment and especially earnings are close to those reported in Table 3, indicating that the majority of the effects on labor market outcomes are indeed caused by the regional economic conditions the graduate faces initially upon graduation. Among cohorts 1996–2004 (column (3) and (4)), there seem to be no effects on unemployment (the χ^2-test rejects the joint significance of the effects on unemployment) and the initial effect on earnings is close to that reported in Table 4. Interestingly, column (3) of Table A6 indicates that the initial unemployment rate produces persistent effects on earnings in later years. This could indeed indicate a true effect on earnings, but it is also possible that the effect on earnings in later years again reflects the fact that some cohorts faced the recent recession which started in 2008 while others did not, and that the inclusion of the effects of later regional economic shocks cannot fully account for this. Another possibility is that, since some of the effects of the further regional unemployment rates on earnings are actually positive instead of negative (not shown), the net effect of the weighted sum of these opposing effects could be positive, thus causing the smaller earnings effects reported in Table 4 for later years. Nevertheless, the veracity of the effects in later years in Table A6 should be treated with caution. In any case, I can conclude that the effects reported earlier are indeed caused mostly due to the unemployment rate a graduate faces immediately upon graduation.

Other Sensitivity and Robustness Tests

Finally, I shortly address three other sensitivity and robustness tests I have conducted. Since none of them changed the conclusions in a significant way, I have chosen not to report their results. First, I dropped all observations with real annual earnings greater than 100,000 euros (corresponding roughly to the 98^{th} percentile in the earnings distribution) and repeated the analyses of Sections 6.2 and 6.3. The point estimates for the most part change very little, signifying that the results are not sensitive to outliers with respect to earnings. Second, I excluded all individuals who obtained a graduate degree from the FLEED sample and repeated the main analyses. Again, the point estimates change very little and the conclusions remain unchanged. Third, I drop all graduates for whom the region of residence in the year of graduation is Uusimaa and repeat the main analyses to see whether the results are sensitive to omitting by far the largest regional graduation cohorts (see Table A8). Overall, the effects on unemployment barely change. For the whole sample, the point estimates of the effects on earnings are roughly 20%–30% smaller. For the depression cohorts of 1988–1995, the effects on earnings are very close to those obtained in Table 4, while for cohorts 1996–2004 the initial effect in the first year is around 28% smaller but for other years the effects are similar. When looking at gender differences, for all cohorts the effects on earnings for the average male graduate are very close to those in Table 5. In contrast, for the average female graduate the effects are less than half of those in Table 5 after the second year since graduation. When looking at cohorts 1996–2004, the earnings effects for the average male graduate can be as much as twice as large after the fourth year than in Table 6. For the average female, the earnings effects are around 40% smaller in the first four years after graduation, but otherwise similar. Despite these differences, it is encouraging that the main conclusions remain.

6.5 Discussion

I conclude this chapter by summing up the empirical findings and relating them to the previous literature. As the results in Tables 3–6 clearly indicate, there are sizable and persistent effects of graduating in adverse economic conditions on labor market outcomes. I also show that these effects are indeed for the most part caused by the economic conditions that graduates face right after graduation and not because of a possibly prolonged recession. As such, my findings with Finnish data complement the previous literature and further re-enforce the robustness of the general conclusion that unlucky cohorts can suffer from adverse initial economic conditions facing them upon graduation, regardless of the institutional environment.

All in all, my main results in Section 6.2 point to larger and more persistent effects than what has been found in many other countries. Most notably, the effects on real

annual earnings I document for the whole sample in Table 3 are much larger than what Liu et al. (2016) find for Norway, arguably the country most comparable to Finland for which a similar analysis has been conducted. Instead, my findings are closer to the evidence from North American studies and for Belgium (cf. Section 2.2 and Table 1). The fact that the effects on real annual earnings are statistically highly significant in all ten years after graduation for the average graduate is rather remarkable. The effect on earnings could possibly persist even much further into the career, given that the initial effect on earnings is only roughly halved by the ninth year (see column (1) of Table 3). Unfortunately, I cannot confirm this since I have opted to include more graduation cohorts and thus limit myself only to the first ten post-graduation years. I also find persistent negative effects on unemployment lasting for the first seven years after graduation for the whole sample. This result is similar to that of Liu et al. (2016) for Norway, but in clear contrast to the mostly short-lived effects found in studies using data from the U.S. and Canada, countries with more flexible labor markets.

As I already emphasized in Chapter 1, Finland is an interesting country on which to conduct a study of this sort, particularly because of the unusually deep 1990s depression. Considering that the countries and time periods which have been studied in the previous literature have not contained a similar economic contraction, studying the 1990s depression is an important addition to the literature. My findings in columns (1) and (2) of Table 4 indicate that the depression hurt the graduates who faced it shortly after graduation in rather equal ways. This indicates that graduating just before the depression started did not really help: the earnings losses became similar after the first two years since graduation. These particularly unlucky cohorts continue to lag behind their luckier counterparts in terms of earnings even ten years after graduation, as can easily be seen in Figure 5. Furthermore, as was seen in Tables 4 and 6, the 1990s depression is driving the negative effects on unemployment that I find for the whole sample. Thus, it seems that highly persistent effects on unemployment can arise even for a population of highly-educated individuals, such as university graduates, in unusually deep economic contractions. Uncovering the underlying heterogeneity and mechanisms at play behind these persistent unemployment effects is a particularly interesting and relevant issue, one that is unfortunately beyond the scope of this thesis.

As was seen Tables 4 and 6, the 1990s depression is also driving the effects on earnings, albeit to a lesser extent. The negative effects on earnings are smaller and relatively short-lived, lasting roughly five years, for the luckier graduation cohorts who did not face the depression. Thus, in an environment of more usual business cycle variation, the effects on earnings of graduating in adverse economic conditions in Finland are closer to those found by e.g Liu et al. (2016) for Norway. Since the effects on earnings for the luckier cohorts arise even though there are no effects on unemployment, it is clear that other mechanisms are responsible for these earnings losses. Since real wages in Finland tend to be more rigid downwards, especially at the microeconomic level (i.e.individual-level) as discussed in Section 4.2, than in other countries that have been studied in the previous

literature, and collective bargains extensively set the wage levels for different tasks (see Section 4.2), the earnings losses may arise through for example task downgrading and skill mismatch. Unfortunately, distinguishing between the relative importance of the alternative channels is beyond the scope of this thesis.

The gender differences I find in this thesis add to the relatively scarce evidence in the existing literature. Many of the notable studies in the existing literature only study male university/college graduates, motivating this choice by the weaker labor market attachment of women (see Oreopoulos et al. 2012, Kahn 2010, and Cockx and Ghirelli 2016). Instead, previous studies have opted to look at differences between e.g. graduates of different fields of study and high- and low-educated individuals. For example, I find that that the effects on earnings found for the post-depression cohorts 1996–2004 are driven largely by males. As already discussed, the smaller earnings losses for female graduates may reflect selection into occupations where the public sector is the primary employer. As Liu et al. (2016) argue, working in the public sector that is more insulated from business cycle variation may help weathering a more turbulent labor market early on in the career. Since men are more likely to work in the private sector, the larger earnings losses for them may also reflect downwards wage adjustments (that are not caused from task downgrading etc.). Although I unfortunately cannot study this with the FLEED data, this hypothesis would be consistent with the findings of Sauramo (2012) on the larger role of wage drift in the private sector.

Finally, I note that my results concerning the gender differences are broadly consistent with the findings of Kondo (2015) who studies the effects of labor market entry conditions across gender (and race) with U.S. survey data from the National Longitudinal Survey of Youth 1979 (NLSY79). She finds smaller effects of facing a recession at labor market entry for women. However, in contrast to my results, in her sample white females seem to be unaffected by labor market entry conditions: she finds no statistically significant effects on real wage rates or employment. She notes that the weaker effects for females may reflect that women have a weaker labor market attachment due to e.g. maternity leaves.

Chapter 7

Conclusion

In this thesis I study the short-term and long-term effects on labor market outcomes of facing adverse economic conditions upon graduation. This study adds to the literature in a couple of ways. First and foremost, to my knowledge I provide the first attempt at studying these questions with Finnish data. Using data on Finnish university graduates who obtained a Master's degree in 1988–2004, I find that facing a high regional unemployment rate in the year of graduation produces sizable and persistent earnings losses for the average graduate. An average graduate who faces a 6 percentage points (corresponding roughly to one standard deviation) higher regional unemployment rate upon graduation has roughly 12.6% lower real annual earnings in the following year after graduation. For the whole sample, the initial effect is halved only after 9–10 years and, remarkably, the effects on earnings are statistically highly significant in each of the first ten years after graduation. The effects on earnings probably persist even several years beyond the first ten years after graduation; however, my choice of limiting to this time period in order to have more graduation cohorts limits my ability to confirm this claim. Nevertheless, my findings on the negative effects on earnings for the whole sample are more persistent than what has been found in similar countries (e.g. for Norway) and are more in keeping with studies using North American and Belgian data.

I also find persistent effects on unemployment for the whole sample which last up to seven years after graduation. These results are consistent with previous findings with Norwegian data, but contrast with the results from North American studies. However, these effects are driven entirely by the cohorts who faced the unusually deep (even by international standards) Finnish economic depression of the early 1990s. Studying this highly unusual time period is the second important addition to the the existing literature. For the cohorts who graduated after the depression generally in more stable economic conditions, I find no effects on unemployment. While the effects on earnings are also driven (albeit to a lesser extent) by the cohorts who faced the depression, the effects remain sizable also for the cohorts who graduated after the depression, but they

are limited to the first five years after graduation. Thus, under more normal business cycle conditions, my results concerning earnings losses are closer to those found by e.g. Liu et al. (2016) for Norway. Since these earnings losses arise while there are no effects on unemployment, it seems that they are driven by other mechanisms. Previous empirical and theoretical literature suggests that these mechanisms include an increase in part-time employment, decrease in the quality of initial employment, task downgrading and skill mismatch in employment, for example. Distinguishing between these mechanisms is essential for future research and for forming sound policy recommendations. Finally, I show that overall my empirical results are insensitive to various alternative model specifications and variable definitions. Furthermore, the results appear not to be seriously biased by selective timing or place of graduation. However, given that the limitations of the data I have do not allow me to definitively rule out the presence selective timing or region of graduation, I view that my findings are likely to provide the lower bounds of the true effects.

All in all, my findings concerning the persistent effects on earnings are valuable by themselves and show that initial economic conditions facing labor market entrants can and do matter in Finland. This is the case even for highly-educated labor market participants such as university graduates. I emphasize that my results only look at *average* effects. As evidence on heterogenous effects, I add to the relatively scarce existing evidence in the literature by finding considerable gender differences. For example, the earnings effects for cohorts who graduated after the 1990s depression are driven largely by male graduates, for whom the average earnings effects are as large and persistent as those found for all cohorts. These gender disparities highlight how differences in e.g. fields of study and employing sector, as well as possibly labor market attachment, can matter greatly in how adverse initial economic conditions affect a graduate's career. However, further heterogeneity analyses on more and less advantaged graduates with respect to e.g. ability and field of study is an important avenue for future research. Moreover, focusing only on highly-educated workers like university graduates would be naïve: research focusing on less-educated workers is also definitely needed in order to form a more complete understanding on how economic conditions early on in the labor market can affect long-term outcomes. Any answers concerning these questions can potentially be used to improve the effectiveness of active labor market policies and various school-to-work programs, for example. I therefore hope that this thesis provides an impetus for more similar research in Finland.

Finally, I address a weakness in the current literature pertaining to the research question in this thesis, namely the link between the magnitude and persistence of the effects and institutional features of the labor market. Many of the studies in the literature (this thesis included) address the link between the effects and institutions only to a limited extent. Since the Finnish labor market can be seen as less flexible than e.g. the U.S. labor market, the fact that I find smaller and less persistent effects on earnings for the post-depression cohorts (1996–2004) than in studies with North American data suggests

that the link between labor market institutions and the effects of facing adverse economic conditions upon labor market entry is more nuanced than what may seem at first. This calls into question the validity of a straightforward link between labor market rigidity and the persistence of the effects. Indeed, as Nickell (1997) for example points out, single institutional features of the labor market (UI system, EPL, wage setting etc.) are not by themselves associated with distinct labor market outcomes (such as a persistently high unemployment rate). Rather, labor market features have complementarities, and it is the combination of these features that together are more indicative of labor market outcomes. Assessing the link between the institutional features of the labor market and the effects of facing adverse economic conditions at labor market entry on labor market outcomes more explicitly is essential, not only for determining the external validity of the results in this literature but also for making the results from the studies more policy-relevant. The result that labor market entrants who face adverse economic conditions can suffer persistent losses is a robust finding at this point, but it is only the starting point for answering the more important question concerning the underlying forces at play. Finding the relevant mechanisms and institutional features responsible for these causal effects is important for devising policies that mitigate them.

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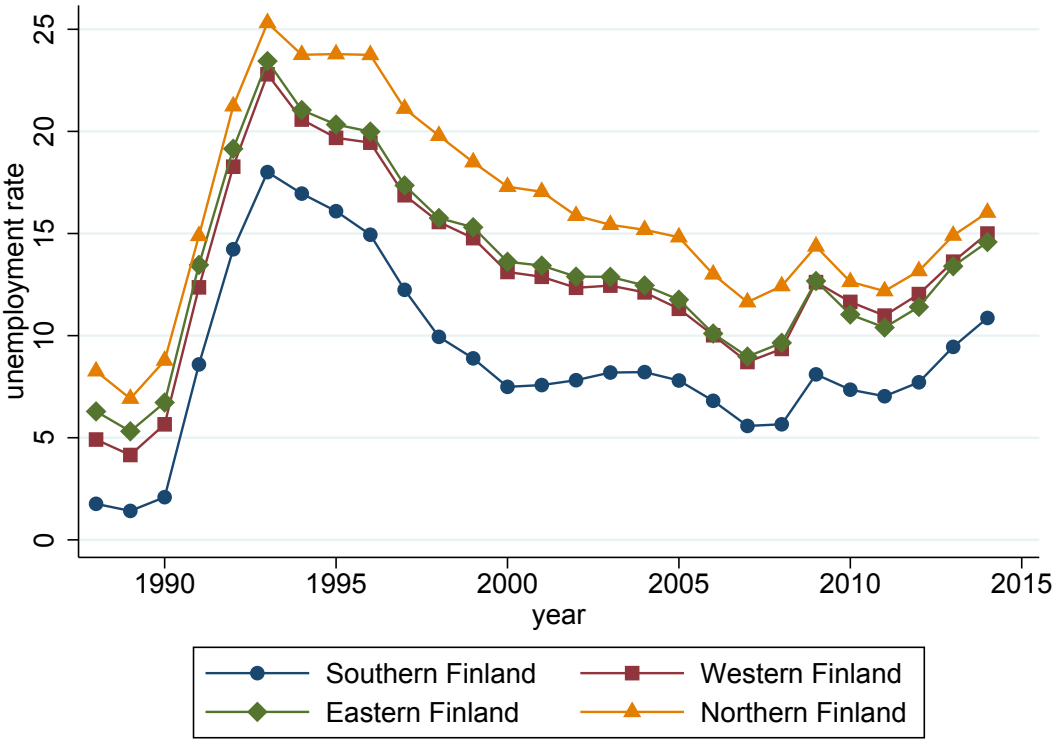
Appendix: Additional Figures and Tables

Figure A1: Major Regional Unemployment Rates, 1988–2014.



Notes: This figure contains the time series of unemployment rates for the time period 1988–2014 for all Finnish major regions (excluding Åland). The unemployment rates are computed using FLEED. See Section 3.1 for information on how these rates are computed.

Table A1: Effects on Group-Specific Means of Unemployment Using Alternative Definitions of Unemployment: All Cohorts vs. Cohorts 1996–2004.

	Cohorts 1988–2004		Cohorts 1996–2004	
	(1)	(2)	(3)	(4)
Effect by Years of Potential Exp. (β_e)	Fraction unemployed $\geq$ 1 month	Fraction unemployed $\geq$ 3 months	Fraction unemployed $\geq$ 1 month	Fraction unemployed $\geq$ 3 months
1	0.0110***	0.0078***	0.0020	-0.0016
	(0.0011)	(0.0008)	(0.0030)	(0.0022)
2	0.0067***	0.0051***	0.0001	-0.0016
	(0.0010)	(0.0007)	(0.0020)	(0.0014)
3	0.0056***	0.0045***	0.0002	-0.0011
	(0.0009)	(0.0007)	(0.0015)	(0.0011)
4	0.0053***	0.0045***	-0.0005	-0.0004
	(0.0009)	(0.0007)	(0.0014)	(0.0011)
5	0.0047***	0.0041***	-0.0012	-0.0007
	(0.0009)	(0.0007)	(0.0014)	(0.0011)
6	0.0038***	0.0031***	-0.0021	-0.0015
	(0.0009)	(0.0007)	(0.0015)	(0.0011)
7	0.0027***	0.0023***	-0.0027*	-0.0015
	(0.0009)	(0.0007)	(0.0014)	(0.0011)
8	0.0017*	0.0015**	-0.0036**	-0.0019*
	(0.0009)	(0.0007)	(0.0014)	(0.0010)
9	0.0010	0.0010	-0.0033**	-0.0012
	(0.0009)	(0.0006)	(0.0013)	(0.0010)
10	0.0004	0.0006	-0.0034**	-0.0016
	(0.0009)	(0.0007)	(0.0013)	(0.0010)
χ^2_{10}-*value*	401.925	244.416	20.290	17.799
p-value	0.000	0.000	0.027	0.058
R^2	0.933	0.888	0.920	0.823
Observations	3060	3060	1620	1620

Notes: This table contains the results from estimating Equation 1 using two alternative outcome variables for unemployment: the group-specific fraction of those having been unemployed for at least (i) one, or (ii) three month(s) during the year. The estimates in Columns (1) and (2) use the whole FLEED sample while the estimates in Columns (3) and (4) only use cohorts 1996–2004. Groups are defined by graduation cohort (year of graduation) and region of residence in the year of graduation. Standard errors clustered by graduation cohort and region of residence in the year of graduation are in parentheses. See Section 6.4 for more discussion.
The χ^2_{10} and p values correspond to the χ^2-test of joint significance $H_0 : \beta_1 = \cdots = \beta_{10} = 0$.
Statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Effects on Obtaining a Graduate Degree.

	Outcome Variable: Has Obtained a Graduate Degree (D_{icr})		
	(1)	(2)	(3)
Effect of Regional UR (β)	0.0006		
	(0.0006)		
Effect of Major Regional UR		0.0011**	
		(0.0005)	
Effect of National UR			0.0008***
			(0.0000)
R^2	0.006	0.000	0.000
Observations	141774	141774	141774

Notes: This table provides the results of estimating Equation 2 using the whole FLEED sample. The specification of Column (1) is exactly Equation 2, while columns (2) and (3) use alternative major regional and national specifications. The specification in Column (2) uses the major regional unemployment rate in the year of graduation as the main regressor and controls for fixed effects of the major region of residence in the year of graduation. The specification of Column (3) uses the national unemployment rate in the year of graduation as the main regressor and only includes graduation cohort fixed effects. Standard errors clustered by (1) graduation cohort and region of residence in the year of graduation, (2) graduation cohort and major region of residence in the year of graduation, and (3) graduation cohort are in parentheses. See Section 6.4 for more discussion.
Statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A3: Effects On Regional Mobility: All Cohorts & Cohorts 1988–1995 vs. 1996–2004.

	Cohorts 1988–2004	Cohorts 1988–1995	Cohorts 1996–2004
Effect by Years of Potential Exp. (β_e)	(1) Fraction Changing Region	(2) Fraction Changing Region	(3) Fraction Changing Region
1	0.0009	0.0016*	0.0089***
	(0.0006)	(0.0009)	(0.0026)
2	0.0010*	0.0020**	0.0048***
	(0.0006)	(0.0008)	(0.0017)
3	0.0004	0.0016*	0.0027**
	(0.0006)	(0.0008)	(0.0013)
4	-0.0000	0.0011	0.0020*
	(0.0005)	(0.0008)	(0.0011)
5	-0.0000	0.0010	0.0018*
	(0.0005)	(0.0007)	(0.0010)
6	0.0000	0.0010	0.0017**
	(0.0005)	(0.0008)	(0.0008)
7	0.0000	0.0009	0.0014*
	(0.0005)	(0.0008)	(0.0009)
8	-0.0000	0.0007	0.0010
	(0.0005)	(0.0008)	(0.0008)
9	-0.0001	0.0005	0.0012
	(0.0005)	(0.0008)	(0.0008)
10	0.0001	0.0007	0.0012
	(0.0005)	(0.0008)	(0.0008)
χ^2_{10}-*value*	21.484	20.347	27.236
p-value	0.018	0.026	0.002
R^2	0.915	0.920	0.928
Observations	3060	1440	1620

Notes: This table contains the results from estimating Equation 1 for group-specific fractions of those changing region. The estimates in Column (1) use the whole FLEED sample while the estimates in Column (2) only use cohorts 1988–1995 and in Column (3) only cohorts 1996–2004. Groups are defined by graduation cohort (year of graduation) and region of residence in the year of graduation. Standard errors clustered by graduation cohort and region of residence in the year of graduation are in parentheses. See Section 6.4 for more discussion.
The χ^2_{10} and p values correspond to the χ^2-test of joint significance $H_0 : \beta_1 = \cdots = \beta_{10} = 0$.
Statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Effects on Group-Specific Means of Logarithmic Real Annual Earnings and Unemployment Using the Major Region Specification: All Cohorts vs. Cohorts 1996–2004.

Effect by Yrs. of Pot. Exp. (β_e)	Cohorts 1988–2004		Cohorts 1996–2004	
	(1) Log Earnings	(2) Unemployment	(3) Log Earnings	(4) Unemployment
1	-0.0254***	0.0038***	-0.0233***	-0.0032***
	(0.0035)	(0.0006)	(0.0081)	(0.0010)
2	-0.0222***	0.0029***	-0.0129*	-0.0018**
	(0.0035)	(0.0006)	(0.0065)	(0.0007)
3	-0.0208***	0.0030***	-0.0090	-0.0014*
	(0.0035)	(0.0006)	(0.0058)	(0.0007)
4	-0.0195***	0.0029***	-0.0064	-0.0018**
	(0.0035)	(0.0006)	(0.0054)	(0.0008)
5	-0.0193***	0.0026***	-0.0040	-0.0017*
	(0.0036)	(0.0006)	(0.0054)	(0.0009)
6	-0.0185***	0.0023***	-0.0023	-0.0013
	(0.0036)	(0.0006)	(0.0050)	(0.0008)
7	-0.0170***	0.0018***	-0.0005	-0.0011
	(0.0036)	(0.0006)	(0.0047)	(0.0008)
8	-0.0160***	0.0013**	0.0013	-0.0014
	(0.0036)	(0.0006)	(0.0045)	(0.0009)
9	-0.0150***	0.0010*	0.0016	-0.0014
	(0.0035)	(0.0006)	(0.0044)	(0.0009)
10	-0.0144***	0.0010*	0.0011	-0.0011
	(0.0035)	(0.0006)	(0.0041)	(0.0008)
χ^2_{10}-*value*	136.213	211.573	49.656	29.244
p-value	0.000	0.000	0.000	0.001
R^2	0.988	0.924	0.992	0.857
Observations	680	680	360	360

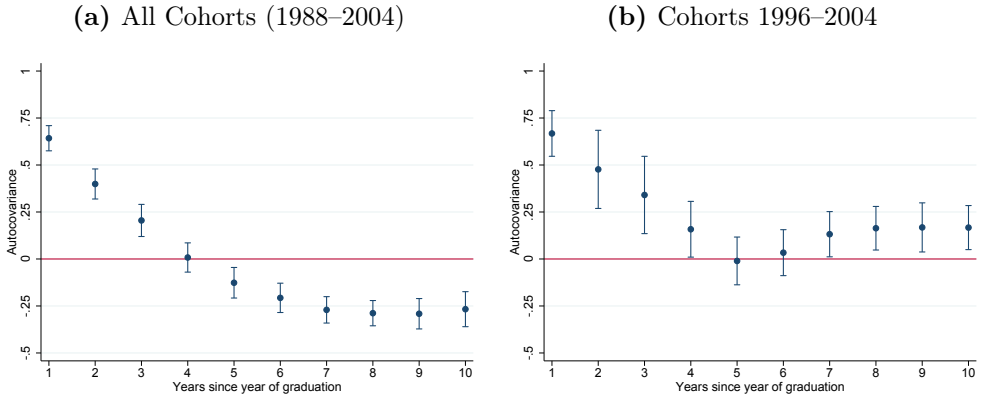
Notes: This table contains the results from estimating a modified version of Equation 1 with major regional data for group-specific means of logarithmic real annual earnings and unemployment using the FLEED sample. More specifically, the main regressor is the unemployment rate of the major region of residence in the year of graduation, and fixed effects for major region of residence in the year of graduation are used instead of region of residence. The estimates in Columns (1) and (2) use the whole FLEED sample while the estimates in Columns (3) and (4) only use cohorts 1996–2004. Groups are defined by graduation cohort (year of graduation) and major region of residence in the year of graduation. Standard errors clustered by graduation cohort and major region of residence in the year of graduation are in parentheses. See Section 6.4 for more discussion.
The χ^2_{10} and p values correspond to the χ^2-test of joint significance $H_0 : \beta_1 = \cdots = \beta_{10} = 0$.
Statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Correlation of Regional Unemployment Rate with Regional Graduation Cohort Size.

	(1) Log Cohort Size	(2) Log Cohort Size	(3) Log Cohort Size
Effect of Reg. UR (β_1)	0.0042	0.0055***	0.0105**
	(0.0106)	(0.0019)	(0.0037)
Grad. Cohort Control	Fixed Effect	Linear Trend	Quadratic Trend
R^2	0.981	0.980	0.980
Observations	306	306	306

Notes: This table provides the results of Equation 3 using the whole FLEED sample. Column (1) fits exactly Equation 3, while the specifications of columns (2) and (3) use a linear/quadratic trend of graduation cohort (year of graduation) instead of graduation cohort fixed effects. Standard errors clustered by region of residence in the year of graduation are in parentheses. See Section 6.4 for more discussion.
Statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure A2: Autocovariance Structure of Regional Unemployment Rates



Notes: This figure shows the autocovariance structure (along with 95% confidence intervals) between the regional unemployment rate in the year of graduation and the regional unemployment rates in subsequent years. The autocovariances are estimated using grouped panel data with annual observations on groups defined by graduation cohort (c), region of residence in the year of graduation (r) and current region of residence (r_e). The estimates are obtained by estimating the following regression model separately for each experience year $e \in \{1, ..., 10\}$:

$$U_{cr_e e} = \alpha + \pi_e U_{cr0} + \phi_t + \theta_r + \theta_{r_e} + \chi_c + u_{crt}, \tag{A1}$$

where α is the constant term, U_{cr0} is the regional unemployment rate in the year of graduation, $U_{cr_e e}$ is the regional unemployment in experience year e, u_{crt} is the error term, and ϕ_t, θ_r, θ_{r_e}, χ_c denote fixed effects with respect to calendar year, region of residence in the year of graduation and current region of residence, respectively. Standard errors are clustered at the level of graduation cohort and region of residence in the year of graduation. The coefficient of interest in Equation A1, π_e, is the autocovariance between the regional unemployment in the year of graduation and the regional unemployment rate in experience year e. See also Supplementary Appendix B of Oreopoulos et al. (2012) and Figure A4 of Liu et al. (2016).

Table A6: Isolated Effect of the Regional Unemployment Rate in the Year of Graduation on Group-Specific Means of Logarithmic Real Annual Earnings and Unemployment: All Cohorts vs. Cohorts 1996–2004.

Effect by	Cohorts 1988–2004		Cohorts 1996–2004	
Yrs. of Pot.	(1)	(2)	(3)	(4)
Exp. (β_e)	Log Earnings	Unemployment	Log Earnings	Unemployment
1	-0.0277***	0.0048***	-0.0244***	-0.0020*
	(0.0025)	(0.0005)	(0.0053)	(0.0011)
2	-0.0146***	0.0025***	-0.0062	-0.0022***
	(0.0023)	(0.0006)	(0.0043)	(0.0008)
3	-0.0140***	0.0028***	-0.0062	-0.0003
	(0.0026)	(0.0006)	(0.0042)	(0.0008)
4	-0.0130***	0.0018***	-0.0088**	-0.0011
	(0.0026)	(0.0005)	(0.0043)	(0.0008)
5	-0.0117***	0.0009	-0.0092**	-0.0011
	(0.0027)	(0.0007)	(0.0045)	(0.0008)
6	-0.0122***	0.0017***	-0.0088**	-0.0001
	(0.0028)	(0.0006)	(0.0044)	(0.0009)
7	-0.0128***	0.0014**	-0.0097**	-0.0005
	(0.0027)	(0.0006)	(0.0045)	(0.0008)
8	-0.0123***	0.0013**	-0.0097**	-0.0006
	(0.0027)	(0.0005)	(0.0046)	(0.0008)
9	-0.0108***	0.0007	-0.0107**	-0.0006
	(0.0026)	(0.0006)	(0.0044)	(0.0008)
10	-0.0097***	0.0008	-0.0128**	0.0007
	(0.0027)	(0.0006)	(0.0051)	(0.0009)
χ^2-*value*	191.064	150.632	73.623	15.437
p-value	0.000	0.000	0.000	0.117
R^2	0.705	0.224	0.691	0.105
Observations	44025	44117	23067	23115

Notes: This table contains the results from estimating Equation 4 for group-specific means of logarithmic real annual earnings and unemployment using the FLEED sample. The estimates in Columns (1) and (2) use the whole FLEED sample while the estimates in Columns (3) and (4) only use cohorts 1996–2004. Groups are defined by graduation cohort (year of graduation), region of residence in the year of graduation, and current region of residence. Standard errors clustered by graduation cohort and region of residence in the year of graduation are in parentheses. See Section 6.4 for more discussion. The χ^2_{10} and p values correspond to the χ^2-test of joint significance $H_0 : \beta_1 = \cdots = \beta_{10} = 0$.
Statistical significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A7: Descriptive Statistics for the FLEED Sample by Years since Graduation

Years since graduation	Statistic/ Obs.	Unemployed	Real annual earnings (in 2012 euros)	Receives unemployment benefits
1	Mean	0.055	29413	0.219
	Std. Dev.	0.228	14412	0.413
	Obs.	141774	140541	141774
2	Mean	0.0451	32651	0.156
	Std. Dev.	0.208	15342	0.363
	Obs.	141774	140450	141774
3	Mean	0.0407	34888	0.131
	Std. Dev.	0.198	17421	0.337
	Obs.	141774	140285	141774
4	Mean	0.0382	36760	0.116
	Std. Dev.	0.192	18337	0.321
	Obs.	141774	140292	141774
5	Mean	0.0354	38707	0.108
	Std. Dev.	0.185	22660	0.31
	Obs.	141774	140219	141774
6	Mean	0.0322	40707	0.0995
	Std. Dev.	0.177	22900	0.299
	Obs.	141774	140219	141774
7	Mean	0.0291	42894	0.0899
	Std. Dev.	0.168	28168	0.286
	Obs.	141774	140274	141774
8	Mean	0.0283	45143	0.0856
	Std. Dev.	0.166	32834	0.28
	Obs.	141774	140371	141774
9	Mean	0.0279	47068	0.085
	Std. Dev.	0.165	30882	0.279
	Obs.	141774	140432	141774
10	Mean	0.029	49166	0.0869
	Std. Dev.	0.168	35028	0.282
	Obs.	141774	140526	141774
Total	Mean	0.0361	39740	0.118
	Std. Dev.	0.187	25586	0.322
	Obs.	1417740	1403609	1417740

Notes: This table gives more detailed descriptive statistics of the main outcome variables by years since graduation for the individuals of the main FLEED sample used in the analyses. See Section 3.1 for information on how the outcome variables are defined and Section 3.2 for detailed information on how the sample is formed.

Table A8: Regional Graduation Cohort Sizes in the FLEED Sample.

Year of Graduation	Uusimaa	Varsinais-Suomi	Satakunta	Kanta-Häme	Pirkanmaa	Päijät-Häme	Kymenlaakso	South Karelia	Etelä-Savo	Pohjois-Savo	North Karelia	Central Finland	South Ostrobothnia	Ostrobothnia	Central Ostrobothnia	North Ostrobothnia	Kainuu	Lapland	**Total**
1988	3088	552	183	150	612	147	159	105	139	273	184	359	161	167	56	488	103	219	7145
1989	2802	593	201	137	613	150	159	106	133	264	155	373	137	185	64	453	88	185	6798
1990	3027	647	200	143	695	152	158	115	132	271	204	336	140	172	62	436	98	171	7159
1991	2936	734	219	162	637	181	162	142	166	259	209	379	147	206	61	556	92	202	7450
1992	2947	720	222	150	670	182	169	131	155	305	223	316	176	215	99	571	98	212	7561
1993	3015	800	221	172	737	164	182	152	147	316	226	433	201	275	71	618	103	237	8070
1994	3048	696	221	152	734	150	157	132	135	298	252	360	200	274	61	581	85	188	7724
1995	3232	789	212	160	732	152	145	148	148	283	210	383	161	258	65	564	87	202	7931
1996	3422	800	214	148	864	156	144	136	153	310	232	386	155	262	73	572	78	202	8307
1997	3660	816	209	163	840	126	134	165	141	291	241	441	160	261	63	606	75	193	8585
1998	3934	827	187	147	917	149	139	144	130	309	204	454	164	251	60	681	85	175	8957
1999	4329	899	198	132	980	127	122	138	111	267	206	486	151	209	71	698	71	152	9347
2000	4090	874	197	128	923	136	129	146	118	278	181	438	123	207	46	778	65	178	9035
2001	4014	854	188	125	954	130	114	149	127	290	238	519	126	224	52	745	70	151	9070
2002	3966	916	208	129	1013	136	130	188	110	300	215	506	122	282	51	762	63	176	9273
2003	4017	976	217	142	1128	162	143	179	138	288	231	547	163	281	55	756	74	180	9677
2004	3886	946	194	163	1100	170	133	183	118	339	246	603	170	276	58	837	79	184	9685
Total	59413	13439	3491	2503	14149	2570	2479	2459	2301	4941	3657	7319	2657	4005	1068	10702	1414	3207	141774

Table A9: Major Regional Graduation Cohort Sizes in the FLEED Sample.

Year of Graduation	Southern Finland	Western Finland	Eastern Finland	Northern Finland	**Total**
1988	3088	1113	1482	1462	7145
1989	2802	1145	1509	1342	6798
1990	3027	1215	1543	1374	7159
1991	2936	1381	1588	1545	7450
1992	2947	1352	1599	1663	7561
1993	3015	1470	1867	1718	8070
1994	3048	1287	1789	1600	7724
1995	3232	1394	1746	1559	7931
1996	3422	1384	1881	1620	8307
1997	3660	1404	1911	1610	8585
1998	3934	1406	1973	1644	8957
1999	4329	1418	2024	1576	9347
2000	4090	1413	1888	1644	9035
2001	4014	1372	2011	1673	9070
2002	3966	1499	2131	1677	9273
2003	4017	1602	2336	1722	9677
2004	3886	1595	2343	1861	9685
Total	59413	23450	31621	27290	141774